

# METHODOLOGY OF THE SOCIAL SCIENCES

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TO  
ALVIN JOHNSON





## Preface

SOME of the most persistent controversies over methods in the social sciences are deeply rooted in general problems of the theory of knowledge, and the solution of these problems is not beyond our reach. To show this is the primary purpose of this book.

Shortly after the publication of my *Methodenlehre der Sozialwissenschaften* (Vienna, 1936), it was suggested that I write a similar book in English, and I started to work on it. But gradually it became a very different book. This is largely due to my study of Dewey's *Logic, the Theory of Inquiry*. While I was strongly impressed by Dewey's analysis of scientific procedure, I could not accept his theory of meaning. This led me to a reconsideration of the problem how the logical analysis of scientific procedure (methodology) is related to deductive logic. I came to the conclusion that methodology must be clearly distinguished from deductive logic and recognized as an autonomous rational discipline. This distinction dominates the argument throughout the book and has partly determined its organization.

The unity of approach thereby attained should facilitate understanding of the essential points for readers who are not familiar with all the issues discussed in the different chapters. Some readers may find it difficult to follow the analysis of probability theories in Chapter VII, but this will not impair their understanding of the rest of the book.

Those who desire more specific information about contem-

porary methods of the social sciences will find it in three works edited respectively by W. F. Ogburn and A. Goldenweiser, by S. A. Rice, and by H. E. Barnes, H. Becker and F. B. Becker; and in two small volumes, *The Social Sciences. Their Relations in Theory and Teaching*, which are the reports of conferences held under the auspices of the Institute of Sociology (London, 1936 and 1937).

For permission to quote from the volumes listed below, grateful acknowledgment is made to the following publishers:

To the Macmillan Company for quotations from *Treatise on Probability* by John Maynard Keynes, and from *Probability, Statistics and Truth* by Richard von Mises.

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I wish to express my deep appreciation of the valuable assistance offered to me by Mr. R. Abel, Mr. J. Altman, Mr. A. Goodman, Dr. C. G. Hempel, and especially by Dr. A. Hofstadter.

The book is dedicated to Alvin Johnson, who led scores of European scholars to the shores of Freedom and guided their first steps in the new world with subtle wisdom. If it were not for Dr. Johnson this book and many others would never have been completed.

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## Introduction

IF Alexander really cut the Gordian knot, this act may very well have been the reason why relations between Aristotle and his pupil were strained in later years. Nothing, indeed, could have been more repugnant to the great philosopher who had fought the *coups de force* in the rhetoric of the Sophists than this violation of the rules of the game, this extermination of a problem instead of its honest solution. But science and philosophy have a Gordian knot of their own, namely, the concatenation of assertions and their grounds, and philosophers have attempted time and again to cut through it by claiming that there is immediate, infallible knowledge of matters of fact forming the unshakable basis of the edifice of empirical science and being neither in need of, nor amenable to, justification on further grounds. Some have even invoked the authority of the Stagirite in support of this thesis by referring to his basic epistemological concept of intuition. However, according to Aristotle (as well as according to Plato), infallible intuition is possible only of universals (essences), not of facts.

All attempts to base empirical science on ultimate grounds conceived as self-evident truths are foredoomed to failure. Yet this insight is but the starting point of a thorough analysis of the meaning of 'ground' in empirical science, which may be regarded as the pivotal issue of methodology (logic of science). Clarifying the meaning of 'ground' is tantamount to determining the criteria for the distinction between warranted and unwar-

ranted assertions, in other words, to explicating the principles of scientific control.

To outline the fundamentals of this analysis and to point to their bearing on some of the most controversial issues of general methodology will be the chief task of the first part of this book. In the second part we shall be concerned with the application of the results obtained to a number of basic methodological problems in social science. Thus it will become apparent that the major methodological controversies in the social sciences cannot be settled if we regard them as peculiar to particular fields of social inquiry or even to the entire domain of social research. One has to discriminate between different levels of generality in the arguments involved and to determine the range of generality of each. It will then be seen that issues of general methodology play an essential part in these controversies and that almost all of the allegedly irreconcilable differences between conflicting views are on this level.

This state of affairs has been obscured by the fact that the issues of general methodology are usually interpreted in terms of opposed philosophical doctrines, e.g. rationalism-empiricism, realism-idealism, subjectivism-objectivism, monism-dualism, determinism-indeterminism, etc. If, then, the adherence of a social scientist to one of these doctrines is taken to be a basic human decision not susceptible of any further objective justification, though explainable in psychological or sociological terms, the idea of objective knowledge in the domain of social science seems to be without substantial foundation.

Our own analysis will proceed along a different path. We shall not assume that the conflicting epistemological doctrines 'behind' the methodological controversies lead to the core of these issues. We hold, rather, that these doctrines themselves have to be properly interpreted and freed from ambiguities in the light of the results attained by an analysis of the fundamental rules of empirical procedure. However, it will facilitate the understanding of our analysis if we preface it by a brief historical sketch primarily concerned with the issue between rationalism and em-

piricism. This will be done with a view toward bringing to the fore the contrast between deductive reasoning (in the strict sense) and empirical procedure. Emphasis upon this contrast, which is closely related to the one between analytic and synthetic propositions (or relations of ideas and matters of fact), will be the guiding principle of our analysis and prove to be the key to the solution of many apparently unrelated methodological problems in natural and social science.

We shall see that fundamental difficulties encountered in the treatment of these problems arise from their elliptical formulation, which does not take account of all their *dimensions of relationality*. To arrive at the complete formulation, it is necessary to refer explicitly to the presupposed rules of empirical procedure. It then becomes clear that many questions apparently concerned with matters of fact are answered by a logical analysis of these rules. Our approach will not comprise all relevant aspects of the methodological issues at stake, but, by revealing their basic structure, it will be of some aid in the complementary analyses on less general levels.





**PART I**  
**General Methodology**



THE distinction between action and perception is one that early forces itself upon the reflection of man. He soon becomes aware that in acting he alters his environment, whereas in perceiving he does not. He realizes further that what he sees, hears, smells, tastes, and touches, when his senses are alert, is determined from the 'outside,' and is thus independent of his will. He need not look in a particular direction, but if he does, what he sees is something definite, regardless of what he may have wished to see. If he goes on to consider that there is an affinity between sense experience and the resistance with which he meets in acting—in so far as feelings, particularly feelings of pain, are determined from the 'outside' in the same way as sense experiences—and if finally he equates perception with knowledge, then the interpretation of knowledge as passive reception of the world lies close at hand. Reference to the retina image seems to provide confirmation of this view. Things, it is concluded, produce their image in the eye, and upon this rests our knowledge of them.

But various experiences cannot be reconciled with this 'copy theory' of knowledge. Above all there are the phenomena that go under the name of 'sensory illusions.' Expectations entertained on the basis of a sense impression are not always fulfilled. Usually, a stick seen as 'broken' is still seen as 'broken' if its position in space is altered. Touch, too, will reveal a break. If, however, a straight stick is dipped into water, and a 'break' is perceived at the point of contact, the actual experience in touch-

ing the stick will differ from what is anticipated under 'normal' conditions. Moreover, the visual phenomenon of a break will disappear as soon as the immersed part of the stick is removed from the water. Thus, the problem arises of establishing what predictions are justified on the basis of given sense phenomena. The hour in which this problem was first conceived was the hour in which the theory of knowledge was born. At first the current interpretation of sense experience still was that it is brought about by the influence of things upon the sense organs and that from the effects thereby produced the true properties of things can be known. Sometimes the cause of the sense phenomenon was regarded as being a unilateral movement emanating from the object, sometimes as the juncture of one movement emanating from the subject with another from the object.

But these interpretations were initially of slight relevance for the development of science. Of the greatest importance, however, were the conceptions developed in order to provide a distinction between 'objective' properties of things and 'merely subjective' phenomena of sense. The latter are characterized by instability in various respects. They are influenced by environmental conditions, and they also differ for different persons under practically identical circumstances. Thus, of two people who dip their hands into the same bowl of water, one may feel the water cold, the other warm, while a person who has previously put one hand in warmer and the other in colder water will simultaneously experience different temperature sensations.

In view of this, we are confronted with the problem of discovering elements that are *invariant* with respect to changes in environmental conditions and differences in the sensory organization of different persons. On the other hand, the knowledge of such invariant elements, taken together with that of various 'accidental factors' (e.g. the position of the observer), permits us to predict the sense phenomena that will appear under these conditions. The goal set for inquiry was thus from the very beginning that of discovering universal invariants and principles of co-variation. Permanence appeared as the criterion of 'true'

reality, and the more permanent thing was called 'more real.' Solids, for instance, were regarded as more real than fluids or gases.

This aspect of analysis is associated with another, the role of reason in the determination of 'true reality.' It is only by comprehending the way in which the two are combined that we can understand the development of methodological thought from Parmenides, Democritus, Plato, and Aristotle through Descartes, Hobbes, Galileo, Locke, Leibniz, Berkeley, and Hume, to and beyond Kant. In what follows we shall direct our chief attention to this point, which is decisive in understanding the history of methodological analysis.

While the copy theory did not go beyond the duality of subject and object and the corresponding duality of activity and passivity, the problem of discovering the permanent elements in change led to the contrast between the receptivity of sense and the spontaneity of reason. Reason shapes the 'material' given by sense under the guidance of ideas lying within itself. The analogy was offered of the creative artist who fashions a block of marble into a statue. But it is well to note that the process by which the object of knowledge is formed is not 'activity' in the sense that it produces changes in the external world. On the other hand, common to both theoretical activity (spontaneity) and practical activity is a teleological factor that determines the direction of action and, moreover, the experience of effort connected with them. A thorough analysis of the impact of the ambiguous notions of activity and passivity on philosophical reflection offers a clue to the understanding of epistemological and ethical doctrines through the ages. The contrasts between form and matter, *a priori* knowledge and *a posteriori* knowledge, mind (or soul) and body, necessity and contingency, good and bad, have been more or less closely related to the contrast between activity and passivity. The issues between realism and idealism, rationalism and empiricism, monism and dualism, in all their varieties, lead back to the questions 'What is given?' and 'How is the given ordered (or transformed) by reason

(mind)?' Here we shall briefly deal only with those problems of philosophy that have a direct bearing upon the interpretation of logic and scientific inquiry. They are found primarily in the controversies between rationalists and empiricists.

The fundamental thesis of the rationalist philosophers is that the key to true being is afforded not by the evidence of sense, but by pure thought, of which logic and mathematics are representative.<sup>1</sup> The following arguments are adduced in support of rationalism:

1. Logical and mathematical knowledge is not subject to the illusions of sense.

2. Results of logical and mathematical analysis are universally and eternally valid; they are not subject to change.

3. The only observations that are precise and intersubjectively valid are those that can be formulated in mathematical terms as the results of counting and measurement.

Because of the influence of Pythagorean modes of thought the appeal to mathematics plays a much greater role in Plato's than in Aristotle's work. Consequently, as modern natural science arose, it could invoke the authority of Plato in its struggle against the Aristotelians. This was of great significance in the philosophical discussions accompanying the rise of modern science; but the fact should not be overlooked that, in essential points, Aristotle's conception of the nature of mathematics is closer than Plato's to the modern view.

The rationality of the world, according to the theological view, is attributable to the plan of the perfectly rational divine Being. Human reason, created in the image of the divine, can understand this plan—at least in part—and thus can comprehend the principles governing the course of events.

The rationalist view permits two interpretations, which lead to different and, in certain respects, even conflicting programs for the acquisition of knowledge. We may contrast them as radical rationalism and critical rationalism. According to radical rationalism only pure reason can comprehend true being and the world process by explicating the elements implicit in the

concept of perfect rationality, such as simplicity, unity, continuity, harmony, determinacy. Thus, the heavenly bodies must be geometrically 'perfect' bodies (spheres) and their orbits 'perfect' curves (circles); their distances from the earth must stand in simple numerical relations. Everything that happens 'must' happen as it actually does. It is 'necessary,' according to rational theology, in the twofold sense that it is inevitable because it is willed by God, and that it serves a specific function in the cosmic scheme. Since the best of all possible worlds is realized through this plan, the harmony of existence and value is also disclosed to reason as it interprets the world scheme. Sense perception can confirm these insights of reason, but the latter do not need such confirmation; nor can they be refuted by sense perception, since reason is the higher faculty.

If we wish to understand the struggle of the great natural scientists of the seventeenth century—especially Galileo—against the Aristotelians, and to decide whether their approach can properly be called anti-rationalistic, we must place primary emphasis upon their demand for control by observation of the supposed truths of reason. This demand is even more relevant than Bacon's plea for the inductive method. Today we know that Galileo and the other great natural scientists of his century were anything but opponents of the deductive method. Galileo discovered his law of falling bodies by deduction from rational ideas.<sup>2</sup> However, he conceived of a rational idea as a hypothesis to be tested by observation and to be dismissed if it cannot withstand this test. We can thus say that natural science is anti-rationalistic in the sense that it does not recognize the derivation of a synthetic proposition from given rational ideas as constituting definitive proof of its empirical validity. But it is rationalistic in so far as it regards rational ideas as guiding principles in making predictions that are then to be tested by observation. Only in this way can we explain the boldness of Galileo's and Newton's assumption that the motions of the stars are subject to the same laws as the motions of falling bodies on earth.

It is therefore not inappropriate to call this attitude either

*critical rationalism* or *critical empiricism*. For, with rationalism, it is critical of sense unguided by reason; and, with empiricism, it is critical of reason unchecked by sense. Once this dual control is properly exercised, then, in the opinion of those scientists, the true nature of things will be disclosed to man, in so far as it is at all accessible to the finite human mind. This point is of decisive importance for understanding the historical relation between methodology and metaphysics. Not only in the Middle Ages but also in the seventeenth century, the goal of inquiry is for the most part interpreted as the unraveling of the mystery of the scheme of creation. The book of nature is to be read just as one deciphers a code. The solution of all problems is predetermined, for it exists in the Mind of God as the meaning of a code exists in the mind of him who composed it. But whether the solution, the correct system of ciphers, has been discovered cannot be definitely established by pure thought alone. Ideas of reason must be confirmed by agreement with the results of observation. The belief that the world has been created by the Infinite Mind and that the creation reflects the perfect rationality of the Creator provides assurance that the quest for truth is not foredoomed to failure.

Perhaps in none of the great natural scientists of the seventeenth century is the influence of this basic religious conviction on the method of inquiry more manifest than in Kepler. But although the linking of rationalistic theology and the investigation of nature can be frequently observed even in the eighteenth century, the tendency to separate them sharply (which had earlier made itself felt in medieval nominalism and the doctrine of the twofold truth) already comes to the fore in Galileo, Kepler's contemporary.

Galileo stresses—usually with polemical intent—that man cannot hope to uncover the inmost essence of nature, but that he can seek, with some prospect of success, to discover uniformities in the flux of phenomena, and that he ought to concentrate his energies upon this task. Newton expresses himself similarly. Nevertheless, this detachment of the interpretation of inquiry



from the idea of a predetermined realm of things-in-themselves was not carried out with complete consistency in the seventeenth century, as is evidenced particularly by Newton's conception of space and time and by his distinction between true and apparent motion.<sup>3</sup>

We may accordingly distinguish two levels of clarity in understanding the meaning of the methods of empirical science. The first is reached as soon as it is realized that knowledge of reality is acquired through systematic observations and their interpretation in terms of theoretical principles. The second is reached when the process of inquiry is freed of all interpretations that ascribe to its results an 'absolute' validity transcending possible human experience.

The development of thought from the first to the second level of clarity can be traced particularly well in the history of the distinction between primary and secondary (objective and subjective) qualities. At first the distinction corresponds to that between (objective) Being and (subjective) appearance. Thus, Democritus says: 'By convention there is sweet, by convention there is bitter, by convention there is warm, by convention there is cold, and by convention there is color.' The truly real consists, for him, of the atoms, unchangeable small particles of substance moving in empty space. He attempts to explain not only physical but also psychical events by means of their attractive and repulsive forces. Here (as later with the great natural scientists and philosophers of the seventeenth century) we already meet with the view that the true nature of things is disclosed in those properties and relations which are measurable (magnitude, figure, motion), and thus directly accessible to mathematical treatment. They afford the key to the interpretation of the universe. There is a famous remark of Galileo's that reads: 'The true book of philosophy is the book of nature which ever lies before our eyes. But it is written in other letters than those of the alphabet; the letters are triangles, squares, circles, spheres, cones, pyramids, and other geometrical figures.'<sup>4</sup>

It is not only between Galileo and Descartes (whose philo-

sophical interpretations of nature have much in common) that there is agreement in principle on this point. Locke's chief work, published some decades later, the basic work of modern empiricism, pursues this line of thought as well. He holds

that the ideas of primary qualities of bodies, are resemblances of them, and their patterns do really exist in the bodies themselves; but the ideas produced in us by these secondary qualities have no resemblance of them at all. There is nothing like our ideas existing in the bodies themselves. They are, in the bodies we denominate from them, only a power to produce those sensations in us: and what is sweet, blue, or warm in idea, is but the certain bulk, figure, and motion of the insensible parts in the bodies themselves, which we call so.<sup>5</sup>

Important as this distinction was as a guiding principle for scientific research, its metaphysical foundation could not withstand thorough critical reflection.

Leibniz, Berkeley, Hume, and Kant discarded the idea of primary qualities as exact likenesses of things existing in themselves. As Kant demonstrated, after having been 'aroused from his dogmatic slumber' by Hume, we can know nothing of things-in-themselves. We are in no way justified in assuming that any phenomena given through the senses are exact likenesses of qualities in things-in-themselves. Consequently, all concepts, including categories such as causality, have meaning only in relation to possible human experience. Their 'transcendent use' leads to antinomies.

Most contemporary philosophical doctrines are agreed that philosophical reflection should not introduce elements transcending possible human experience. Philosophy, it is now realized, is analysis of experience. But this fact cannot be interpreted as a complete victory of sensationalism over rationalism. For the insight of the rationalist philosophers, that experience is not simply given through sense, is today common to most philosophical doctrines.

The problem of disclosing the 'activity of the mind,' i.e. the

problem of making explicit the structure of experience, has remained. What has been refuted is *radical* rationalism, i.e. the idea of the 'supremacy of reason over sense,' according to which indubitable judgments about reality can be obtained by pure conceptual analysis.

To understand what rational analysis of experience means, we must recognize that there are different degrees of clarity of thought and that it is possible to pass from a lower to a higher. *Docta ignorantia* means that one does not 'really'—that is, not quite clearly—know what one knows. It may be accompanied by the desire to attain 'real' knowledge, that is, to reach clarity. Socrates' method aims at leading his partners in a discussion towards this goal by suggesting thorough reflection to them and by keeping this reflection in the right path. Such reflection reveals, in the first place, inconsistencies of thought that have to be removed, and, in the second place, implicit tendencies according to which some elements may be regarded as essential and others as non-essential in a certain context. The essential elements are often hidden. The process of clarification may tentatively be likened to the attempt to revive a faint remembrance (Plato's *anamnesis*). But there are important differences between these two processes. Though we cannot engage here in a thorough analysis of this fundamental philosophical problem, some remarks pertaining to it will be required for the logical arguments to be offered in the following chapters.

Clarification of a meaning always implies separating it from other meanings that are usually associated with it. Thus, the idea of an integer may be associated by primitive man with that of his fingers, which he uses in counting. At a somewhat advanced stage it may still be associated with digits and with the decimal system. He who knows how to transform the decimal system into another number system—e.g. the dyadic system—may still associate the idea of integers with that of a power series. But it can be shown that all this is 'unessential' for mathematics. This does not mean that it is irrelevant for the practice of counting or calculating. Primitive man was really unable to count without

using his fingers, and the actual development of mathematics would have been virtually impossible without the Arabic (Indian) algorism. But this is not the point. If we say that the features mentioned above are not essential for mathematics, we mean that they need not be *referred to* in a mathematical proof. It is to the lasting credit of Berkeley that he stressed this point in his criticism of Locke's theory of abstract ideas, when he analyzed the example of a triangle 'neither equilateral, nor scalenon, nor equicrural.'<sup>6</sup> But the arguments he offers in this context by no means prove the impossibility of abstract meanings. Abstract meanings are made explicit in determining the elements essential in a particular demonstration.

Generally speaking, a conceptual or propositional meaning is essential for (*presupposed* in) a context of thought if it has to be referred to—explicitly or implicitly—in this context. In stating that a thing of a particular kind is at a particular place at a given time, we presuppose the meaning of 'space' and 'time' and of the properties characteristic of this kind of thing. The meaning of an 'abstract concept' is presupposed in the meaning of the 'concrete concept' from which it is abstracted.

The fact that 'concrete thinking' precedes 'abstract thinking' in mental development is no longer paradoxical when we consider the various degrees of clarity of thinking. In thinking about a concrete object of a given kind, the properties that are constitutive of this kind are implied, but they need not be *clearly* conceived.

Failure to realize this leads to the genetic fallacy, which consists in confounding analysis of meanings with causal explanation of facts, a fallacy that has time and again blocked the path to a thorough understanding of logical and philosophical problems. All efforts toward solving these problems are doomed unless we apprehend the sense of the question 'What do we mean by . . . ?'—in other words, unless we grasp the peculiar character of the process of clarification.

This point will be further developed in the following chapter.

WORDS and sentences are linguistic signs for meanings. A person, the speaker, communicates with another person by means of these signs. In understanding them, the hearer applies, more or less consciously, a system of rules that give to particular acoustical phenomena the function of symbols for particular thoughts. If acoustical phenomena do not have this function, we call them meaningless. This term is always relative to a given language. A combination of sounds that is meaningless in English may be meaningful in French or Esperanto. To translate words and sentences from a language  $L_1$  into another language  $L_2$  is to determine those symbols in  $L_2$  that have the same meaning as the symbols in  $L_1$ . Obviously it is not essential for a science whether it is formulated in one language or another if both are able to convey the meanings concerned. Physics treated in an English textbook is the same as that treated in any translation of it. Accordingly, we must distinguish between meanings and the symbols representing them, and we must take heed that reference to meaning is essential for language. The question whether or how far thinking occurs apart from association with linguistic signs is irrelevant in this context.

This state of affairs has been obscured, however, by the view that abstract meanings are somehow 'created' by language, by the process of assigning a common name to things similar in some respect. Modern nominalism, as conceived, for instance, by Hume, was directed not only against the *metaphysical* hy-

postatization of abstract ideas by the conceptual realists (Platonists), but also against their *psychological* hypostatization by Locke, although it accepted the fundamentals of Locke's sensationalism. It tried to dispose of meanings by interpreting them in terms of functions of language. In doing so, it seemed to remove the difficulties connected with the concept of mind in Locke's philosophy and to pave the way for scientific investigation of linguistic phenomena. However, this explanation is of no avail for logical analysis since it fails to observe that 'language' is defined in terms of 'meaning.' To trace back 'meanings' to 'language' is like tracing back 'straight lines' to 'triangles.' This inversion of logical order is characteristic of all attempts to 'explain' meanings in terms of facts.<sup>1</sup>

In what follows we shall, in accordance with prevailing usage, understand by 'term' and 'sentence' linguistic signs for meanings; by 'concept' and 'proposition,' meanings regardless of the type of linguistic signs by which they are represented. Accordingly, we may say that terms and sentences *have* meanings, whereas concepts and propositions *are* meanings.<sup>2</sup>

Conceptual meanings are either *elementary* or *composite*. But even elementary meanings are not devoid of structure. This structure is referred to when one speaks of the topological properties of space (e.g. its tridimensionality), or of properties of colors (hue, brightness) or sounds (pitch, volume).

Here, however, we find ourselves confronted with the question: *How are the elementary meanings given?* Two answers have been offered in the history of philosophy that appear to be exclusive of each other. One is that they are given by 'experience,' ultimately reducible to sense data; the other, that they are given by 'reason' or 'intuition,' prior to all experience. According to the latter view, the faculty of reason or intuition is innate, an essential character of the soul. The simile corresponding to the first view is that of the soul as a blank sheet of paper on which experience writes (Locke).<sup>3</sup> According to the second doctrine it may be compared to a marble block disclosing certain veins that cannot be neglected by the artist (Leibniz).<sup>4</sup> Unfortunately, the

issue has almost always been obscured by the failure to distinguish between logical and genetic priority. The philosophical problem is not to give a causal explanation of the appearance of ideas in the soul (mind), but to clarify the presuppositions implicit in thought and to arrange them in their proper logical order. In describing a sense experience, we have to characterize it in general terms and thereby to presuppose meanings (e.g. 'blue'). Any attempt, therefore, to determine the meaning of 'blue' in terms of a particular sense experience is futile. An 'ostensive definition' is not a definition. Nor do we fare better if we refer to 'reason' or 'intuition' as the source of universals. No such faculty can be characterized without presupposing the meaning of its performance.

However, this does not imply that the question: 'How are the elementary meanings given?' is devoid of all significance. I believe that phenomenology has disclosed the nature of the underlying problems.<sup>5</sup> But these are problems neither of empirical science nor of logical analysis in the strict sense. Here we have to presuppose such meanings.

Elementary meanings may be divided into two classes, 'space' and 'time' forming one class, and 'qualities' the other. Qualities are given in 'systems' (systems of colors, sounds, etc.). Different qualities of a given system are 'incompatible' with one another in the sense that they cannot be assigned to the same space-time point. Thus 'red and blue at place *p* at time *t*' is presupposed as impossible. Analysis of systems of qualities reveals a hierarchical structure of meanings ranging from the *infima species* (e.g. a specific shade of blue observable at a particular place) to the *summum genus* (e.g. color). It should be noted that the relation between species and genus within such a system is different from the relation between the *summum genus* and the formal category under which it falls. Other types of internal relations hold between qualities of different systems. There is, for example, no color where there is no extension.<sup>6</sup> Analytic propositions make such meanings explicit.

In what follows we shall confine ourselves to synthetic proposi-

tions about the physical world. Any *synthetic proposition*—affirmative or negative—is a *restriction of the frame of possibilities*; as a determination it excludes other determinations.<sup>7</sup> An example is the assignment of a quality to a particular place and time. By stating, "There is blue at place  $p$  at time  $t$ ," we exclude all other colors from this place in space and time. By negating this statement, we exclude the color blue from this place and time. Therefore, the following three cases must be distinguished:

1. Restriction of the frame of possibilities in a particular way.
2. Restriction of the frame of possibilities in a way incompatible with the former.
3. Restriction neither in the first nor in the second way (lack of determination). Retraction of a previous assertion  $p_1$  *annuls* the restriction of the frame of possibilities established by  $p_1$  but does not exclude it.

We now come to an observation that is basic for the analyses in the following chapter, namely, that 'synthetic proposition' should not be defined in terms of 'truth' and 'falsity.'

In an examination of the opposite view we must refer to its historical root. Traditional logical theory has been decidedly influenced by Aristotle's way of introducing the term 'proposition.'<sup>8</sup> Aristotle starts by defining 'sentence.' 'A sentence is a significant portion of speech, some parts of which have an independent meaning, that is to say, as an utterance, though not as the expression of any positive judgment . . . Every sentence has meaning, not as being the natural means by which a physical faculty is realized, but, as we have said, by convention. Yet every sentence is not a proposition; only such are propositions as have in them either truth or falsity. Thus a prayer is a sentence, but is neither true nor false.' He adds the programmatic resolution: 'Let us therefore dismiss all other types of sentence but the proposition, for this last concerns our present inquiry, whereas the investigation of the others belongs rather to the study of rhetoric or of poetry.'

Entering thus into the definition of 'proposition,' the concepts



of truth and falsity seem to be of fundamental significance for deductive logic. Closer analysis, however, reveals that they have no legitimate place there. To prove this we shall start with a comparative analysis of propositions in Aristotle's sense and such sentences as, according to him, are not propositions. Our point will be that every sentence in Aristotle's sense implies propositional meaning. If one declares that, in contrast to indicative sentences, imperatives or questions or prayers can be neither true nor false, one emphasizes that the proposition implicit in them is usually not related to a procedure of control concerning its validity. But this is not to say that they are not susceptible to such control. We shall refer to imperatives only,<sup>9</sup> but the argument applies to all types of sentences that, according to Aristotle, are not propositions.

On the prevailing interpretation of the imperative, our intention in uttering it is not to assert a fact but to give expression to a wish or a volition, in order to induce other people to act in a particular way—to obey the command. Hence, it is argued, only the contrast between effective and ineffective is applicable here, not that between true and false. We shall now examine the adequacy of this interpretation.

In contrasting imperatives and propositions, one usually views the fact of communication as essential for the former but not for the latter. But this prevents a correct approach to the problem. We shall therefore contrast a communicated imperative with a communicated indicative sentence.

It is necessary to distinguish first between the *meaning* and the *purposes* of a communication. A communication is usually made in order to promote a set of more or less closely connected ends. If I say to a friend at a street crossing, 'There comes an auto,' it is my purpose to make him aware of the danger, so that he shall be able to adapt himself to the situation and thus avoid harm. But attainment of the *proximate* end of the communication, namely, that of letting my friend know something, which includes the understanding of a particular meaning, is required for the attainment of all the other ends concerned. This holds

equally for assertions and imperatives. The fact that in the imperative the proximate end (the conveying of information) recedes into the background while attention is focused upon the further ends (influence upon the behavior of another person) should not mislead us. We must ask: 'What does A let B *know* when he communicates a command to him? What is the meaning of the information conveyed to him?' It is: 'I wish to induce you to behave in a particular way.' Expressed in objective terms, the propositional meaning implicit in an imperative directed to B by A is then: A wishes to induce B to behave in a particular way (to perform or abstain from a particular action).

No logician will deny that this proposition *could* be examined in regard to its truth. But usually there will be no intention to do so. It will be taken for granted that the person who utters the imperative really wishes what he declares himself to wish, and any control in this direction will be regarded as beside the point. In this sense one may state: An imperative is neither true nor false, since one does not intend to apply to it a procedure of verification or falsification. But the proposition included in the imperative could be found true or false (or, as we shall prefer to say, valid or countervaild) if such a process were applied to it. Sometimes such a control is, indeed, suggested, e.g. if a person receiving a command or a request reacts by saying, 'You do not really want me to do so,' or if the addressee of a question (a request to convey some information to the man who utters it) declares that this is only a rhetorical question.

There may be no intention to control a given proposition even when it appears in the linguistic form of an indicative sentence. Such an intention does not exist in the case of propositions contained in the literary creations of fancy. Historical descriptions or narrations of past events are intended to be subject to scientific control; romances or novels are not. If an assertion within the framework of a historical inquiry does not withstand control, we say that it is false, but not that it is 'mere fancy.' This would imply that such a proposition should neither be subject to control nor function in the control of other propositions. To say that

mere fancy by definition cannot be true or false thus means that it is not intended to be related to a process of verification. But control is not precluded by the *meaning* of such sentences.

We shall now show that the concepts of truth and falsity need not be referred to in defining the *logical relations of synthetic propositions*.

In the textbooks of logic these relations are usually taken to be truth-relations.<sup>10</sup> For example, two propositions  $p_1$  and  $p_2$  are said to be contrary when, if  $p_1$  is true,  $p_2$  is false. But such a formulation is inappropriate, for it seems to suggest that in order to discover whether two propositions are contrary we have to inquire about their truth or falsity, whereas their meaning alone determines the relation.

The way in which two given propositions restrict the frame of possibilities uniquely determines their logical relations, as will be apparent from the following definitions.

We say that a proposition  $p_2$  is *deducible from* (logically contained in) a proposition  $p_1$ , if every possibility excluded by  $p_2$  is also excluded by  $p_1$ .

Two propositions are called *contrary* if each excludes the possibilities selected by the other.

Two propositions are called *contradictory* if each of them excludes those and only those possibilities that are selected by the other.

Two propositions are called *subcontrary* if their negations are contrary.

We have not included a definition of equivalence as the relation of mutual deducibility. Logical equivalence is identity, so that we have here only one proposition, not two. To establish the equivalence of different sentences means to establish the identity of their meanings. However, this is likely to be overlooked if we fail to distinguish between 'sentence' and 'proposition.'

The logical theory of the *forms of propositions* no more presupposes the concept of truth than does the theory of the relations among propositions. One of the fundamental distinctions

here is that between universal and singular propositions, and we must clearly understand the meaning of 'universal proposition.' We shall choose a simple example to illustrate it. In the synthetic proposition 'All fish breathe through gills,' 'fish' is defined by a set of traits not including gill-breathing; otherwise our proposition would not be synthetic. Let us call those traits  $q_1$ ,  $q_2$ , . . .  $q_m$  and the trait of gill-breathing  $q_n$ . We then arrive at the following formulation: 'At all places and times the trait  $q_n$  is existent if the set of traits  $q_1$ ,  $q_2$ , . . .  $q_m$  exists.'

It has not been duly remarked in traditional logic that, besides constancy and unrestricted free variability in space and time, it is necessary to take into consideration the form of *free variability within certain bounds*. This restriction may relate either to space alone (all spatial dimensions or only some), or to time alone, or to time and space.

In making an assertion about all bodies, we do not imply any restriction of spatio-temporal variability. But we do when we make an assertion in social science about all men. For in speaking of 'men,' we make implicit reference to the earth as their habitation, as well as (more or less vague) reference to their genesis in a particular period of the history of the earth.

Accordingly, we shall differentiate between *unrestricted* and *restricted* universal propositions. In a narrow sense these terms apply only to synthetic propositions. But we shall sometimes use them in a broader sense, applying them to analytic propositions as well.

The negation of a universal proposition is called an existential proposition. The negation of an unrestricted universal proposition may be called an unrestricted existential proposition; and the negation of a restricted universal proposition, a restricted existential proposition.

It is generally recognized that the concept of truth does not enter into the distinction between universal and singular propositions. The matter seems to be different with respect to the distinction between simple and compound propositions. Propositions may consist of other propositions connected by 'logical constants,'

e.g. 'and' (conjunction), 'or' (alternation), 'if—then' (implication). Such propositions are called compound propositions. Propositions not so composed are called simple propositions. A compound proposition is uniquely determined by the propositions of which it is composed and the mode of its composition. If we know how each of the component propositions restricts the frame of possibilities, then we know also how the compound proposition restricts the frame of possibilities. This is referred to in the 'truth-tables' of mathematical logic where the truth-value (truth or falsity) of a compound proposition is determined by the truth-values of its component propositions. But here again the nucleus of the truth-relation is a relation among propositional meanings.<sup>11</sup>

To summarize the preceding observations: A synthetic proposition restricts the 'given' (presupposed) frame of possibilities. Neither in the definition of 'synthetic proposition' nor in that of the logical relations among synthetic propositions or of the various forms of synthetic propositions is there any reference to the concept of truth. But every synthetic proposition can be subjected to a process of empirical control. That is why synthetic propositions are usually called empirical propositions. However, since empirical control of synthetic propositions need not always be intended, as our discussion of imperatives and of fancy has shown, we shall not regard the two terms as synonyms.

So-called *synthetic propositions a priori* prove, on closer analysis, to be *analytic*.

Inexactness in using terms often leads to a confusion of synthetic and analytic propositions. Suppose we define 'fish' as follows: 'A fish is an animal (the meaning of "animal" is presupposed as "given") that possesses the traits  $q_1, q_2, \dots q_n$ .' Let the possession of organs of vision, but not that of gills, be included among  $q_1, q_2, \dots q_n$ . If we then compare the two propositions 'All fish have organs of vision' and 'All fish breathe through gills' without explicit reference to the definition, they appear to be of the same kind. In fact, however, the first is analytic whereas the second is synthetic. Only the second proposition restricts the frame of possibilities; the first is im-

plicit in the definition of 'fish,' since we have given the name 'fish' only to animals possessing organs of vision. Confusion of both types of propositions in scientific thought is often brought about by shifts in the meanings of terms, which accompany scientific progress.

In order to avoid this error, it is important to make it clear that the basic structure of synthetic propositions is determined by the cleavage between space-time positions and qualities. On the one hand, there are the determinations corresponding to the questions 'where?' and 'when?' and, on the other, those corresponding to the question 'how?' The space-time region is the 'logical subject'; the quality, the 'logical predicate.' This appears clearly in propositions of the simplest form, such as 'There is blue at place  $p$  at time  $t$ .'

A grammar adequate to the logical structure of propositions would strictly mark this cleavage between space-time positions and qualities. To be sure, there is a tendency in grammar to reflect adequately (isomorphically) this dual structure of meaning. But this tendency conflicts with the tendency to reflect the temporal order of the process of acquiring knowledge, with its selection of relevant factors and the advance from what is taken for granted to what is newly found. Hence, the differentiation between qualities or relations that are included in the definition of the subject and those that are predicated of the subject. But this distinction has no correlate in the logical structure of propositional meanings. In the proposition 'This fish is red,' the qualities by which 'fish' is defined as well as the quality 'red' are assigned to the space-time region designated by 'this.' The qualities are conjugate, and conjunction is a symmetrical relation. In the structure of meaning, therefore, none of the qualities has a special status. This is obscured by the grammatical structure of the sentence, in which 'red' is the predicate whereas all other qualities concerned are included in the subject term.

The foregoing remarks are not to be interpreted as stating that language is primarily responsible for confusions of thought.

We must not forget that lack of distinction in language is, in most cases, the consequence of unclear thought. Still, it has to be conceded that a language may more or less easily lure those who employ it into disregarding certain logical distinctions. And it cannot be denied that an established linguistic habit tends to perpetuate those errors of reasoning that are 'embedded' in it. This applies particularly to the lack of distinction between the logical order of meanings and the temporal order of acts of apprehension. Indeed, this confusion is largely responsible for the controversy concerning the order of priority between quality-concepts and thing-concepts or relation-concepts, which we shall now discuss.

The concepts of space, time, and quality are logically prior to thing-concepts, since they are elements of thing-concepts. Every thing is a thing of a particular kind and has, as such, certain constitutive qualities. The statement that things are given by experience and that quality-concepts are established by a subsequent process of abstraction refers to the temporal order of psychical acts. Being a process of clarification, abstraction does not establish a logical order; rather, it makes this order explicit.

Confusion here is heightened by shifts in the meaning of terms. Thus, 'gold' is first defined in terms of the spatio-temporal co-existence of a number of observable qualities, such as a particular color and hardness. In the course of scientific progress, however, things are classified according to 'more relevant' properties (including relations). Today the chemist defines 'gold' in terms of the periodic system of elements, and thus no longer refers to the directly observable qualities of gold. We may express this more formally: Certain spatio-temporal configurations of qualities  $q_1, q_2, q_3$ , which form a starting point of inquiry, appear regularly together (at the same time or at *regular* intervals) with other spatio-temporal configurations of qualities  $q_4, q_5, q_6$ . A certain thing-concept, e.g. 'gold,' corresponds to the configuration  $q_1, q_2, q_3$ . But, as inquiry goes on, it may be found that the connection between  $q_1, q_2$ , and  $q_4, q_5$  is of more consequence for

inquiry than that between  $q_1$ ,  $q_2$ , and  $q_3$ . This may be then taken into account by redefining the thing-concept in terms of  $q_1$ ,  $q_2$ ,  $q_4$ ,  $q_5$ . Retention of an old name for a new meaning is indicative of the historical development of the process of inquiry, in which the major scientific relevance of the new meaning has been established.

Essential for the pre-scientific thing-concept is the idea of a more or less permanent coexistence of given qualities. That is why solid bodies appear as the models of things. In everyday language we call a piece of furniture in a room a thing, but not the air in the room. But this distinction has little significance in science.

The realization that a thing-concept cannot be resolved into quality-concepts (that a thing is 'more than its qualities') is one root of metaphysical speculations concerning 'substance,' and the experience of the 'unity of the person' is another. Moreover, scientific experience regarding the conservation of matter is referred to in these speculations—a typical confusion of relations of meanings with matters of fact. Whatever may be the significance of Hume's critique of the concept of substance, it by no means solves all the pertinent problems posed by Aristotle, and the same must be said of the admirable treatment of this issue in Kant's *Critique of Pure Reason*. Husserl's analysis of the logical constitution of thing-concepts, as developed in the third of his *Logische Untersuchungen*, represents an important advance beyond Hume and Kant in so far as he brings to light the structural interrelations among the various classes of qualities, for example, color and extension.<sup>12</sup>

Failure to distinguish clearly between analysis of meanings and description or explanation of facts is also at the root of the difficulties that have arisen in the analysis of the concept of relation. These difficulties reached a climax when Bradley maintained that the relation-concept leads to antinomies. He declared that it is unintelligible how the relation can stand to the qualities.



If it is nothing to the qualities, then they are not related at all; and, if so, as we saw, they have ceased to be qualities, and their relation is a nonentity. But if it is to be something to them, then clearly we now shall require a new connecting relation. For the relation hardly can be the mere adjective of one or both of its terms; or, at least, as such it seems indefensible.<sup>13</sup>

Criticizing this Neo-Hegelian theory of truth, Russell has attacked the underlying 'axiom of internal relations' which he formulates as follows: 'Every relation is grounded in the natures of the related terms.' Russell's analysis<sup>14</sup> has provoked a spirited discussion.<sup>15</sup>

We cannot refer here to the bearing of this issue on the fundamental tenets of the Neo-Hegelian doctrine but must confine ourselves to those aspects of it which are significant for the analysis of scientific procedure.

In what follows we shall not speak of the relations among terms or qualities, but rather of the relations among propositions, or among things or facts. But it is easily seen that this does not involve a shifting of the problem. Instead of saying: An internal relation  $R$  holds between two qualities  $q_1$  and  $q_2$ , we can say: The relation  $R$  holds between any two things  $t_1$ , having the quality  $q_1$ , and  $t_2$ , having the quality  $q_2$ , by reason of their having these qualities. It is then seen that the proposition 'the relation  $R$  holds between  $t_1$  and  $t_2$ ' can be derived from the prepositions ' $t_1$  has the quality  $q_1$ ' and ' $t_2$  has the quality  $q_2$ .'

Proceeding now to 'external relations,' we have to ask: What relation among propositions corresponds to what is called an external relation among facts? Let  $f_1$  and  $f_2$  be two different facts designated by two logically independent propositions  $p_1$  and  $p_2$ , and  $p_3$  a synthetic universal proposition from which, in combination with  $p_1$ ,  $p_2$  can be deduced. Then we shall say that  $p_2$  stands in an external relation to  $p_1$  in terms of  $p_3$ . If it is also possible to deduce  $p_1$  from  $p_3$  and  $p_2$ , we shall say that  $p_1$  and  $p_2$  stand in external relations to each other in terms of  $p_3$ .

Generally speaking, the logical core of an external relation

(understood in this sense) among  $n$  facts is an internal relation that includes  $n$  singular propositions and one or more synthetic universal propositions.

It is therefore elliptical to speak simply of an external relation between two propositions, without mentioning the third proposition that is implicitly referred to. It will become apparent in the following chapters that this point is of basic significance for the logical analysis of scientific procedure.

This aspect of the problem of external relations falls entirely within the scope of deductive logic; no reference to the empirical validity of the propositions is implied. But we still have to make clear in what sense external relations can be regarded as contingent in contrast to necessary internal relations. In the first place, it is possible to restrict the use of the term 'external relation' so that it applies only to established facts and laws, i.e. to *accepted* singular propositions and synthetic universal propositions. This restriction is implicitly presupposed in setting the problem of *finding* external relations among facts of given kinds, i.e. of establishing laws enabling us to explain or predict facts on the basis of other facts. We are thus concerned only with valid propositions, i.e. with propositions that have been verified in an empirical procedure. Given the propositions designating the facts, we have to find—more precisely, to establish empirically—the law that connects them. This suggests the idea that a logical relation among propositions can be established in empirical procedure. However, this temporal aspect of inquiry does not enter into the timeless logical relations among propositions, nor does it make any difference for deductive logic whether or not the propositions concerned are empirically valid.

But the distinction between necessary and contingent relations points to an even more important methodological issue, which we shall discuss in greater detail in Chapter VI. There we shall show that a law, i.e. a universal proposition from which, in combination with singular propositions, predictions can be obtained, need not be a *synthetic* proposition. It may be a rule of scientific procedure stating that the acceptance of a prediction is war-

ranted on the basis of a fact (designated by a singular proposition). In this case, as we shall see, we have an 'external relation' of a different kind.

By observing the foregoing distinctions and the one between the temporal aspect of the process of thinking and the timeless structure of meanings, we come to the heart of the controversy whether relations are created by the mind or whether they are in things.<sup>16</sup> Considering that relations are supposedly not immediately given, as sense qualities are taken to be, that a mental effort is needed to find them, and that it is a matter of methodological resolution what kind of relations one is trying to establish, some philosophers expressed the view that relations are creations of the mind.

Some of the factual psychological problems involved have been successfully dealt with by Gestalt psychologists (Wertheimer, Koffka, Köhler, Lewin). It has been established by these scientists that apprehension of spatial and temporal relations is included in sense perception. The problems that are the prime concern of the philosopher in this context are, however, (a) whether there are in propositions pertaining to the physical world specific relational meanings not reducible to the meanings of space, time, and qualities; (b) whether, granted (a), the relational meaning points in a similar way to 'experience' as do the quality meanings. Considering that all internal relations among qualities are derivable from the meanings of the quality-concepts and further that all other relations in the physical domain are concerned with spatio-temporal locations of qualities, we arrive at a negative answer to question (a), so that question (b) does not come into play at all.

In this chapter we have attempted to make it clear that logical analysis of the meaning of synthetic propositions does not imply any reference to their validity (truth or falsity). But the thesis that logic is analysis of meanings seems to be incompatible with the 'logic of calculi,' which apparently operates with 'meaningless signs.' That there is no such incompatibility is clearly seen once it is realized that operations with the formulae of a

calculus are applications of given rules (transformation rules of the calculus) in terms of which the meaning of 'proved formula' is defined.<sup>17</sup> The formulae as such have no meaning. They acquire meaning only when the calculus is 'interpreted.' But the rules by which 'logical calculus' and 'proved formula' are defined do have meaning. That a machine can 'produce' such proofs means that it can bring forth series of formulae correct according to the rules. Logical analysis, however, does not consist in the 'producing' of formulae but in making it clear that they fall into a class (provable formulae of the calculus) defined in terms of pre-supposed rules. Accordingly, it is analysis of meanings.

It should finally be noted that the validity of the results of deduction does not have its ultimate source in the rules. It is not in terms of any rules that propositions include or exclude one another. Rather, inclusion or exclusion is given with the meaning, and the adequacy of rules of deductive inference, e.g. of moods of the syllogism, can be proved by analysis of propositional meanings. However, such a proof is not possible for the rules of empirical procedure, to which we turn in the following chapters.

### III

### Pre-scientific and Scientific Thinking

IN discussions of the role of science in modern civilization it is frequently maintained that science has radically transformed the thinking habits of men. If this is taken to mean only that the results of scientific inquiry have uprooted firmly established beliefs and created a vast number of opposite ones, it may be readily admitted. Sometimes, however, it is claimed that ways of thinking in modern scientific inquiry are fundamentally different from, and often contrary to, the familiar kind of thinking in everyday life.

This thesis requires closer examination. It is associated with the usual way of explaining the meaning of science by contrasting it with pre-scientific thinking. There are good reasons for this view. It is suggested by historical study of the development of science. It brings to the fore the obstacles that have blocked the path of science and the glorious manner in which they have been overcome. It is thus conducive to genuine enthusiasm for this great human enterprise and aids us in understanding the spirit of the modern age.

On the other hand, emphasis upon this difference is likely to lead to inadequate interpretation of scientific procedure by diverting attention from the elements common to science and pre-scientific thinking and by exaggerating their diversity. Thus, it is often declared that scientific concepts are fundamentally different from the concepts of pre-scientific thinking. This, however, is an untenable view. The scientist cannot create new fun-

damental meanings like those of shapes or colors or sounds, nor can he create combinations of such meanings. What he actually does when he introduces a term is to select a particular combination of fundamental meanings that he regards as significant in the context of his inquiry and to give it a new name. That is why the definitions in a science are indicative of its method, and controversies among scientists over definitions usually reflect their disagreement about the methods to be employed.

This state of affairs is obscured by an erroneous interpretation of the meaning of definition in science according to which the scientist creates new objects that have no existence in the external world, but only in his mind. In consequence, questions arise whether such objects as atoms or electrons are real or merely fictitious, and the view is widespread that the realm of science is populated by shadowy mental constructs whereas the world of common sense consists of familiar visible and tangible things.

If this view were sound, there would indeed be a fundamental difference between pre-scientific and scientific concepts. But it is not. No term can have any significance for physical science unless its meaning is related in some way or other to perceptual material. On the other hand, there is no meaning, either in scientific thinking or in pre-scientific thinking, that does not imply a mental construction (synthesis). What we mean when we say that stones are real things is, broadly speaking, that under suitable conditions it can be decided *by an observational test* whether the proposition 'There is a stone at a certain place at a given time' should be accepted. The same holds for an electron. We can, of course, define the term 'real' in such a way that stones, but not electrons, will fall under our definition. However, such a definition would not be in accordance with the actual use of the term. It might be objected that there is the essential difference that stones are directly observable whereas electrons are not. This objection is met by making explicit the assumptions implicit in the 'direct observation' of stones. The directly observed

thing is by no means simply given by sensation devoid of spontaneity.

This point is recognized as soon as one considers the implicit reference to the future in statements about observed things. If I state on the basis of a visual observation that a stone at present exists at a certain place, I imply an indefinite number of anticipations of possible future experiences, mine as well as others'. For example, if I shut my eyes and open them again after a while, looking in the same direction, I shall have similar visual experiences, and so would any other man with normal sight occupying this position. Similar reflections show that reference to the past is included in every assertion of the present existence of a thing.

Our conclusion, then, is that atoms and electrons are real in about the same sense as the familiar things of everyday life. In both instances we have to distinguish between matter and form, sensory content and mental construction. To be sure, an electron is not immediately given in sensation, but neither is a stone. If this seems strange, it is so only because there is a strong inclination to confuse psychological simplicity with logical (structural) simplicity, though they have been distinguished time and again since the days of Aristotle. Since no mental effort is involved in the observation of a stone, the structural complexity of the process remains unnoticed. This error is, incidentally, the chief source of the time-honored opinion that it is the business of philosophers to make easy matters appear difficult.

An objection to the preceding argument must, however, be expected. Noted physicists, it may be said, have declared that the concepts of modern physics are nothing but sets of mathematical formulae. They have warned against popular misinterpretations of modern physics according to which an electron is conceived of as, say, a small billiard ball. In reply to this objection we may point out that while it is, of course, misleading to conceive of an electron as a small solid body, it is also misleading to say that it is a set of mathematical formulae. Apart from the confusion of signs and their meanings, we are here confronted

with the fallacy of confounding pure and applied mathematics, a fallacy that is responsible for many misinterpretations of scientific thinking. There are rather intricate problems involved here, but the fundamental point is easily understood.

Let us suppose a noted astronomer announces over the radio that a great cosmic catastrophe has just occurred and that consequently the sun will rise a minute later tomorrow than hitherto expected. After listening to his explanation, we may find it more or less convincing and may wait eagerly to see whether his prediction will be confirmed. But if our astronomer should declare that two times two equals five henceforth as a result of a cosmic catastrophe, we should doubt his sanity instead.

How can the difference of attitude in these two cases be justified? The plain answer to this question is that arithmetic has nothing to do with real events. If we assert that the sun will rise at a particular time of a particular day, we state that one of several different possibilities will be actualized. Such a selection or restriction of possibilities is the core of the meaning of any assertion about reality. To declare that the opposite of a proposition is impossible or inconceivable implies that this proposition does not contain any *determination* of reality. Closer analysis reveals that the propositions of pure mathematics, like  $2 \times 2 = 4$ , are analytic propositions. Applied mathematics, on the other hand, consists of propositions asserting that there exist particular numerical relations among real phenomena. Such propositions restrict the frame of possibilities and are therefore synthetic. For instance, by stating that a certain tree is 100 feet high, we establish, broadly speaking, a numerical relation between the length of a measuring rod and the height of a tree. Such statements are, of course, subject to empirical tests and may be refuted by it. The same holds for physical laws. The fact that they refer to numerical relations by no means implies that they are irrefutable. For example, a statement that the force of attraction between two bodies varies inversely as the cube of their distance is no less 'mathematical' than the statement that it varies inversely as the square of their distance. Empirical tests



will lead us to decide which of these two statements, if either, should be accepted. Pure mathematics cannot decide this issue. It can only aid in the testing of such statements by making explicit their logical consequences. This is the meaning of Einstein's famous dictum: 'In so far as mathematics is about reality it is not certain, and in so far as it is certain it is not about reality.'<sup>1</sup> When some physicists declare that an electron is a set of mathematical formulae, they overlook the fact that such formulae acquire significance for physics only by being related to observable phenomena. The establishment of such a relation is sometimes called a co-ordinating definition, a term suggested by Reichenbach. P. W. Bridgman's book *The Logic of Modern Physics*<sup>2</sup> contains an admirable analysis of some of the problems involved here.

Scientific thinking has often been characterized as 'quantitative' and contrasted with 'qualitative' pre-scientific thinking, with the implication that science yields absolutely certain knowledge by establishing mathematical laws, while common sense can lead only to more or less probable beliefs. The preceding argument makes it clear that this view is untenable. There is yet another preconception implied in it which is also closely associated with our topic, namely, the idea that by dealing with measurable magnitudes science has freed itself from the subjective elements of sense experience.

But in fact it cannot be consistently maintained that the subjective factors of sense perception are excluded by introducing measurable magnitudes. Let us examine the statement that, by measuring temperature with the aid of a thermometer, we rid ourselves of subjective factors. The first objection to this view is that we depend on sense observation in determining the length of the mercury column in the thermometer. Whatever self-registering instruments may be devised, there must always be a man using his eyes when he takes the pointer-reading, which means that sense perception is not entirely excluded.

The second objection is that the very sense data from which we have turned away come into play again as soon as we apply

the results of the pointer-reading to predictions concerning our feeling of warmth. Our reason for looking at the thermometer may be to find out whether we shall be likely to feel cold if we leave home without an overcoat. People living before Galileo had to lean out of their windows to learn this. We avoid this trouble by regarding the length of a mercury column in a thermometer as an indication of a prospective feeling of warmth or cold under certain conditions. But this relation involves all the vagueness and subjectivity from which we had wanted to free ourselves. Generally speaking, measurement of intensive magnitudes implies discovery of an empirical relation between them and extensive magnitudes. This relation is far from being precise and need not remain unaltered if persons and situations change.

The misleading expression that qualities are 'transformed' into quantities in taking such measurements is connected with the idea that quantities rather than qualities reveal the true nature of the universe. As a stimulus to the discovery of quantitative relations, this idea has been of the greatest moment for the development of modern natural science. Particular types of numerical relations, e.g. such as are expressible in terms of differential equations of the second order, were found to be invariant in the course of changing events and to prevail in apparently quite diverse fields of phenomena. The result was the establishment of a highly integrated body of physical laws.

This idea seems to justify the claim that there is no science where there is no measurement and that psychology and social research are still largely in a pre-scientific stage. We shall examine this thesis more thoroughly in Part II. Only one point related to it may be mentioned here. Many of its proponents believe that the terms 'mathematical methods' and 'methods of measurement' are synonymous. However, this is not so. It suffices merely to mention topology, which, incidentally, may play an increasingly important role in the future in psychology and the social sciences. Topology is concerned with those properties of geometrical objects that are invariant with respect to one-one continuous transformations.

Reference to this mathematical discipline may well serve to draw our attention again to the all-important distinction between the logical order of meanings and the temporal order of inquiry. Topology is only a hundred years old, while metrical geometry had already reached a remarkable level 2300 years ago. Fundamental topological concepts like that of dimension<sup>3</sup> seemed to be so obviously given by intuition that mathematicians did not bother analyzing them until the middle of the nineteenth century. As soon as they did, however, it became clear that these concepts are of foremost significance for a deeper understanding of metrical geometry, at which we arrive by determining its place in a hierarchy of geometries with topology as its base.<sup>4</sup>

Similar considerations apply to the *logical analysis of empirical science*. Charles Peirce has stressed how extremely difficult it is 'to bring our attention to elements of experience which are continually present.'<sup>5</sup> But scientists may find that it is worth while to face these difficulties.

The basic elements of empirical procedure are common to pre-scientific and scientific thinking, and there is indeed no sharp line of demarcation between them. The following points are essential:

1. Empirical procedure is concerned with a division of synthetic propositions into two disjunctive classes, accepted propositions and non-accepted propositions.
2. A decision by which the status of a proposition is changed, i.e. by which its transfer from one class into the other is performed, must not be arbitrary; grounds must be offered to justify it.
3. Only propositions accepted at the time of a decision can be such grounds.
4. No decision is irreversible; no proposition that has been accepted is exempt from the possibility of elimination at a later stage of inquiry. In other words, there is a permanent control of every accepted proposition.
5. No decision must lead to a body of knowledge containing two incompatible propositions.

6. No proposition falling within the subject matter of a science must be *a priori* excluded from acceptance.

7. Propositions stating results of observations (sense observations or self-observations) play a key-role as grounds for the acceptance of singular propositions.

Points 1 and 2 are superordinate to points 3-7. Taken together they give the most general characterization of scientific thinking, namely, that it is a *process of classifying and reclassifying propositions by placing them into either of two disjunctive classes in accordance with presupposed rules.*<sup>6</sup>

The term 'ground' is frequently used with ambiguous meaning. Suppose we say of two accepted propositions  $p_1$  and  $p_2$ , that they are (sufficient) grounds for the acceptance of the proposition  $p_3$ . This may be taken to mean either that  $p_3$  is deducible from  $p_1$  and  $p_2$  or that  $p_3$  can be accepted because  $p_1$  and  $p_2$  are accepted, even though  $p_3$  is not deducible from  $p_1$  and  $p_2$ . Understood in the first sense, the term 'ground' is not related to the logic of empirical procedure. To show that a proposition  $p_3$  can be deduced from two propositions  $p_1$  and  $p_2$  is to make clear that it does not entail any determination of reality—any restriction of the frame of possibilities—not entailed in  $p_1$  or  $p_2$ . In other words,  $p_3$  is not *new* (in a logical sense) relatively to  $p_1$  and  $p_2$ . We do not *accept*  $p_3$  by deducing it from the accepted propositions  $p_1$  and  $p_2$ . We *recognize*, rather, that it has already been accepted in accepting  $p_1$  and  $p_2$ . No change is made in the corpus of a science by a process of deduction, though clarity concerning the logical implications of accepted propositions may be of the greatest moment for scientific progress. In this sense, the meaning of 'ground' in deductive logic is not intrinsically related to that of empirical procedure. We shall therefore use the term 'ground' exclusively in the second sense, where it does have such a relation to empirical procedure. It is then apparent that we cannot determine merely by a logical analysis of  $p_1$  and  $p_2$  whether  $p_1$  is a ground of  $p_2$ . Rules of empirical procedure have to be referred to.

We have listed the fundamental properties of such systems

of rules and mentioned that they apply to pre-scientific as well as to scientific thinking. Henceforth we shall confine ourselves to the analysis of *scientific* procedure.

By 'subject matter' or 'theme' of a science we understand a certain frame of meaning, and the question whether a given proposition  $p$  falls within the subject matter or theme of a given science  $S$ , say physics, is decided by analysis of meaning. Only if the answer is in the affirmative can an empirical procedure decide whether  $p$  may be incorporated into  $S$ .

Our next task is to show that an adequate definition of 'science' is in terms of rules of procedure. A brief historical remark will be appropriate. Since Plato, Aristotle, and Euclid, science has been interpreted by the majority of philosophers as a deductive system of propositions. According to the original idea, there stands at the head of the system a set of self-evident propositions (axioms), which neither require nor are susceptible of proof. From these, other propositions (theorems) are derived by a process of reasoning (deduction). Coupled with this idea was the argument that, granted the possibility of genuine knowledge, the regress of proofs must terminate at a definite point. But this would be incompatible with the methodological principle that no proposition is exempt from control. Therefore, the theory had to be modified so as to state that every science of reality is to be viewed as a hypothetico-deductive system. Thus, the postulate of the systematic unity of science was retained, but it was admitted that no given system of propositions can claim ultimate validity.

However, the definition of 'science' as a hypothetico-deductive system of propositions is not satisfactory either. This definition implies that the set of all valid propositions established by inquiry within a certain domain cannot constitute the corpus of a science unless they form a deductive system. This would mean that most of the existing branches of knowledge could no longer be called sciences, and even the few 'sciences' then remaining would lose the title as soon as new experiences led to a 'disturbance' of the system.

It will not do to reply to this stricture that a deductive system is not affected in its intrinsic validity by the question whether it is appropriate for the description of a specific domain of fact. For applicability as well as logical consistency is demanded in the empirical sciences. To be sure, explanation of all the facts in a field of inquiry by laws that can be ordered into the hierarchy of a deductive system is an *ideal* of inquiry; but this leaves us in the dark concerning the conditions a proposition must satisfy in order to be included in the corpus of a science and to remain there. Such a definition will not tell us, for example, why the observational test is to be considered an essential element in scientific procedure.

It follows that a specific science, say physics, should be defined in terms of rules of procedure rather than as a system of propositions representing our knowledge at a given time. Obviously, both the science of Galileo and Newton and the science of Einstein and Bohr are called 'physics,' and we do not regard this as a mere equivocation.

It may be objected that the definition of 'science' in terms of rules of procedure is incompatible with the view that a science *consists* of a set of propositions, which might be taken to mean that it is completely determined by them. To meet this objection it will be well to distinguish between the *structure* of a science as defined in terms of the rules of procedure and the *corpus* of the science at a given time, i.e. the set of propositions accepted at this time in accordance with the rules of procedure of the science. Since the corpus of a science is selected in accordance with these rules, it cannot be taken to determine a science without reference to them.<sup>7</sup>

But now we are faced with the apparently all-important question: 'How are these rules given?' The confusion of different types of problems indicated by this question has been a major obstacle to the understanding of the meaning of methodology.

In declaring that rules of scientific procedure are 'given,' the logician means that they are presupposed in judging whether scientific decisions are correct, or, in other words, that 'correct-

ness of scientific decisions' is defined in terms of the rules. 'Givenness' has here the same meaning that it has for the mathematician when he says, 'Given the diameter of a circle: to determine its circumference.' No question regarding the origin of the rules is involved.

Three issues concerning the givenness of the rules must carefully be separated from one another, namely:

1. How the rules came to be accepted,
2. How acceptance of the rules can be justified, and
3. How the meaning of the rules is made explicit.

We need not enter here into the question how the rules of inquiry came to be acknowledged. Biological, psychological, and sociological hypotheses would have to be conjoined and appraised. We should have to refer to the role that magic played in the development of technology and the role that technology played in the development of science. We should have to show how successful habits of thought were gradually recognized as standards and progressively refined. But the meaning of the assertion that certain habits of thought were successful requires further clarification. We thus arrive at our second question, that of the justification of the rules.

It might first be suggested that habits of thought in general and habits of inquiry in particular are considered successful if the actions occasioned by them lead to desired results. In fact, however, what matters is not the desirability, but the predictability, of consequences. The 'success' of a theory is established by fulfilled predictions derived from it, regardless of whether the predicted events are desirable.

The most obvious criterion of the success of methods (habits) of thought is indeed the confirmation by observational tests of the results to which they have led. But to grant this is not to admit that thought and perception are completely independent of each other. The sharp contrast between perception and thought has been one of the major hindrances to an adequate analysis of the procedure of empirical science. Observation is but one way of verifying a proposition among other related controls. To be

sure, observational results possess a certain primacy over theories, but the very meaning of this primacy can be determined only within the whole framework of a system of rules of empirical procedure. We shall treat this point more fully in the next chapter.

The system of rules of scientific procedure must not be interpreted as a set of means for the attainment of ends defined without reference to such rules. But there are some kinds of rules that contain a reference to ends of inquiry. We shall contrast them, as *preference* rules of procedure, with those that do not contain a reference to such ends, and call the latter *basic* rules of procedure. The analogy with a game of chess may be of aid in understanding this point. The goal of checkmating the opponent is defined in terms of basic rules of chess. On the other hand, it is presupposed in the preference rules of chess, by which 'better' and 'worse' moves are distinguished.

This analogy brings another important point to our attention. In a game of chess each move, i.e. each change in the situation on the chessboard, is considered in isolation if one asks whether it agrees with the basic rules of chess in terms of which 'permitted' moves of the different chessmen are defined. But in evaluating a move, i.e. in appraising it in terms of preference rules, we have to relate it to other moves, some already made and some anticipated. In a similar way, each change in the corpus of a science must be considered in isolation if its correctness in terms of the basic rules is to be examined, but the appraisal of its correctness in terms of preference rules requires that it be related to other procedural decisions. Whereas, however, all the ends of the chess player are subordinate to the final end of checkmating his opponent, which terminates the game, there is no 'end of the game' indicated by the rules of scientific procedure. But we may draw an analogy between the goals of the chess player and the scientist's proximate goals, the solving of given problems.

There is finally one more point to be considered in this context. If we interpret all rules of scientific procedure as technical rules, which indicate useful means for attaining given goals of



inquiry, then we are led to the view that they may be replaced by any others that are taken to be more conducive to the attainment of these goals. However, as soon as it is seen that the goals themselves are defined in terms of basic rules of procedure, it becomes clear that such a view is untenable.

It might then even seem as if the assumption that any basic rule can be changed were self-contradictory. Since the structure of a science is defined in terms of given rules, it may be held that a science defined in terms of changed rules would be a different science. But this argument overshoots the mark. It is sound in so far as it emphasizes that we cannot speak of a 'change' in the procedural rules of a science in the same sense as we do of a change in the corpus of the science. However, there is a sense in which the system of procedural rules may be said to be flexible. To make this point clear, we shall draw upon the analogy between the structure of science and the structure of a legal order. Whether a given norm belongs to a particular legal order will depend upon whether the norm has been 'put into effect' in a legal way. What we mean by 'a legal way' is defined in terms of other norms, which may themselves be altered in accordance with the constitution. If we analyze what we mean by 'unity of a legal order,' we recognize that it is compatible not only with unlimited alteration of substantive law but also with limited alteration of the rules of legislation. In a similar way, it is found by analysis of what we mean by 'unity (or identity) of a science' that it is compatible with alterations of rules of procedure. Thus, we should not regard the unity of scientific procedure as destroyed if certain rules of induction were replaced by others. This point will be discussed further in Chapter VI.

However, certain fundamental properties of the system of rules—above all, those that we have listed—must be considered invariable. For example, a system of procedural rules referring to propositions about the external world that did not include the observational test would not be called a system of scientific rules.

The question whether the rules of scientific procedure can be altered is not one of fact, but rather concerns the definition of

scientific procedure. It becomes a question of fact only indirectly, through the implicit demand that the definition accord with the (rectified) use of the term 'science.'

We thus arrive at the third question mentioned above, namely, how the rules of procedure can be made explicit. This is not simply a matter of psychological description; it implies 'idealization' (rectification). Scientists not infrequently violate the rules of logic. But in such cases—we may assume—they would usually acknowledge that they had erred if their attention were called to the fact. It would also appear that in general those scientists who are recognized as superior seldom infringe upon these norms. We can say, accordingly, that the rules of logic are acknowledged by scientists even when they violate them. Furthermore, there exists among scientists the tendency to harmonize conflicting rules. This tendency is taken into account in making explicit the rules of inquiry.

Unfortunately, the pertinent problems have usually been regarded as issues concerning the relative ranks of methodology and science. Can methodology legislate to science—so the question is often put—or must it content itself with describing the procedure of the scientist? This controversy is easily understood in the light of the development of modern science in its struggle with rationalistic metaphysics, but it has become obsolete. The issue of the relation between methodology and science is comprehended in the general issue of clarification of meanings.

Sometimes this problem is posed by asking whether the rules of method are given *a priori* or whether they are conventions. To answer this question we must first be clear about what we mean by (a) 'a priori,' (b) 'convention.' If we associate with the term 'a priori' the idea of irrefutable statements about reality, then the rules of the sciences are not *a priori*. For since they are definitions, they do not make any assertions about reality at all. Furthermore, they are not *a priori* in the sense of being derivable (as are the rules of deductive reasoning) from the propositions to which they apply. But the rules are *a priori* in the sense that—again because they make no assertions about reality—they can-

not be refuted by 'experience' (observation); and they are *a priori* for science because 'science' is defined in terms of them.

On the other hand, they are, as definitions, conventions concerning the use of (methodological) terms. Moreover, they are conventions in the sense that scientists are bound by them. If, however, we associate with the term 'convention' the additional meaning that conventions can be replaced by others without thereby changing anything 'essential'—as is the case, for example, with respect to the units of measurement (cm, g, sec) in physics, or the decimal system in arithmetic—then the basic rules of scientific procedure are certainly not conventions. In fact, they determine precisely what is essential for science.

In discussing the fundamental problems of scientific method, we need not sharply discriminate between the rules of procedure of different sciences. The invariable fundamental properties we have referred to are common to all sciences *qua* sciences; and only if the pertinent methodological problems are clearly posed can systematic analysis of the relation between different sciences, e.g. natural sciences and social sciences, be performed. We shall discuss these problems more thoroughly in the second part of this book. The disclosure of isomorphisms has proved to be of the greatest significance for mathematics; it may be of considerable significance for the empirical sciences as well.

## IV

## The Basic Rules of Scientific Procedure

WE may formulate some of the results of our analysis in the preceding chapter in a slightly different way.

From the point of view of the logician, the procedure of an empirical science consists in the acceptance or elimination of propositions in accordance with given rules. Whatever else the scientist may do, whether he looks through microscopes or telescopes, vaccinates guinea pigs, deciphers hieroglyphics, or studies market reports, his activities will result in changing the corpus of his science either by incorporating propositions that did not previously belong to it or by eliminating propositions that previously did. Such a change in the corpus of science may be called a *scientific decision*.

The scientist must not make a decision arbitrarily. To do so would be a violation of the rules to which he has implicitly pledged himself by engaging in scientific inquiry.<sup>1</sup> He must give grounds for each decision, i.e. he must show that it is permissible (correct) in terms of the presupposed rules of scientific procedure. In other words, the rules of scientific procedure state the conditions for an exemption from the general prohibition against changing the corpus of a science. A suitable name for this prohibition would be 'the methodological principle of sufficient reason,' for it can be regarded as the methodological correlate of the metaphysical principle bearing this name.<sup>2</sup>

It follows that there are no specific rules of scientific procedure forbidding a change in the corpus of a science. Procedural

rules may either permit such a change without prescribing it or prescribe it. For example, the rules of induction contain the conditions for the acceptance of universal propositions, but the scientist is never bound to incorporate such propositions; he is always free to wait for additional corroboration. On the other hand, in testing hitherto accepted universal propositions, he cannot simply ignore a disparity between his predictions and the results of observation. He is usually bound to accept these results, i.e. to incorporate the propositions stating them.

A brief analysis of the conception of logic as a normative science will aid us in understanding the character of the rules of scientific procedure. What does it mean to say that logic does not teach us how man really thinks, but how he ought to think—that it consists of the rules or norms of thought?

A norm is a maxim that governs the behavior of the person who seeks to comply with it. However, for the person who appraises human behavior in terms of the norm, it is a criterion for the correctness of this behavior. In other words, it is for him a definition, or part of a definition, of 'correct behavior of a particular type.' Correct thinking is defined in terms of agreement with the rules of logic, just as correct speech is defined in terms of agreement with the rules of grammar, or legal behavior in terms of agreement with given norms of positive law. The grammarian's task is to state whether a certain type of speech is in accordance with the presupposed rules of grammar; the jurist's task is to state whether human actions of a given type are in accordance with presupposed legal rules; the logician's task is to state whether thinking of a given type is in accordance with presupposed rules of logic. Questions concerning the genesis of these norms, or their significance for practical purposes, or their more or less frequent violation are not to the point here. The problem in all three cases is: Given (a) a set of definitions concerning human behavior and (b) human behavior of a certain type, to decide whether or not such behavior falls under the definition.

It is plain from what has been said that it is inconsistent to

deny the givenness of rules and at the same time to declare the objective control of every scientific decision to be essential for science. For the rules are presupposed in control.

There is, of course, room for discussion regarding how much consensus there is among scientists with respect to the rules of method. It could be shown, for example, that there is no complete agreement concerning the rules of induction. Yet when it is declared that on the basis of present knowledge there is inductive justification for the acceptance of some universal proposition in science, rules of induction are presupposed as 'objectively given.' This is to say that the meaning of 'inductive justification' is made clear only by referring to such a system of rules. Similarly, it may be doubted whether it is possible to give a definition of 'customary law' that would agree with a general use of the term. Yet if the assertion that a certain norm has become customary law is to be examined, a definition of 'customary law' must be presupposed.

As soon as it is understood that concepts such as 'correct,' 'grounded,' 'control,' 'confirmation' are relational concepts presupposing a system of procedural rules, these conclusions become a matter of course. But this point is seldom clearly understood because truth and falsity are regarded as immanent properties of propositions. Consequently, the controls are interpreted as means of discovering which of the two truth-values is possessed by the proposition. Such an interpretation prevents us from realizing the different possible kinds of status that a proposition may have in empirical procedure. As we shall presently show, fifteen different kinds of status can be distinguished in terms of a basic principle of classification.

Let  $p$  be a proposition pertaining to the theme of a science  $S$ . We can distinguish at first 6 ( $2 \times 3$ ) different procedural relations between  $p$  and  $S$ . At any stage of inquiry  $p$  may either belong or not belong to  $S$ . If  $p$  does not belong to  $S$ , there are three possibilities:

1.  $p$  must not be accepted in  $S$ .
2.  $p$  may, but need not, be accepted in  $S$ .

3.  $p$  must be accepted in  $S$ .

If  $p$  belongs to  $S$ , there are again three possible cases, namely:

4.  $p$  must not be eliminated from  $S$ .

5.  $p$  may, but need not, be eliminated from  $S$ .

6.  $p$  must be eliminated from  $S$ .

If we further consider that the status of a proposition is affected by the status of its negation, we arrive at 36 ( $6 \times 6$ ) possibilities. Dropping those that may lead to contradiction, fifteen remain, as may be seen in the following scheme.

Let  $\bar{p}$  be any proposition incompatible with  $p$ . By correlating the six possibilities for  $p$  with the compatible possibilities for  $\bar{p}$ , we obtain:

$p:1$	....	$\bar{p}: 1,2,3,4,5,6$
$p:2$	.....	$\bar{p}: 1,6$
$p:3$	...	$\bar{p}: 1,6$
$p:4$	.....	$\bar{p}: 1$
$p:5$	..	$\bar{p}: 1$
$p:6$	...	$\bar{p}: 1,2,3$

The suggestion might be made that the rules of procedure be classified in accordance with the six possibilities previously mentioned. We should then have to distinguish three classes of rules: those that, under certain conditions, prohibit; those that permit without prescribing; and those that prescribe, the acceptance or elimination of propositions. But this would not be in order since, as we have just noted, there are no rules of procedure that forbid a change in the corpus of science. In stating that reasons must be given for every change in the corpus of science we establish a general prohibition. The rules of procedure exhaustively determine the exceptions to the general prohibition. This fundamental point may be illuminated by the analogy with the regulations of a criminal code, where the general principle has been established that no one can be convicted as a criminal unless he has violated the norms of the code. There are no *specific* norms in such a code forbidding punishment.

We have already noted in the last chapter that the grounds

for a scientific decision, as determined by the rules of procedure, are exclusively propositions accepted at the time of the decision. For any given science *S* they may be divided into three groups, namely:

- (a) propositions belonging to *S*
- (b) propositions belonging to another science, and
- (c) accepted protocol propositions<sup>3</sup> of the form,  
 'The person N.N. made a certain observation, e.g. saw blue at a certain place at time *t*.'

Of course, only a few of these propositions are significant for a particular scientific decision.

It will be helpful at this stage of our analysis to introduce two technical terms. We shall call the totality of synthetic propositions accepted at a particular time the *scientific situation* at this time,<sup>4</sup> and we shall call a *step in S* a scientific decision that is correct in terms of the basic procedural rules of *S* and is not the compound of two or more correct scientific decisions.

Accordingly, every correct scientific decision in *S* either is a single step or consists of a number of steps in *S*. The step is the procedural element defined in terms of basic rules of procedure.

The question whether a given decision is a step is answered by reference to the situation at the time of decision. That the scientific situation changes in the course of time suggests that there can be no 'timeless' logic of scientific procedure. But this view is untenable. 'Correctness of a scientific decision' is defined in terms of a scientific situation, namely, that prevailing at the time of the decision. But the definition contains no reference to this time. What matters is only the kind of situation. Whether a scientific situation of a particular kind is established at a given time is a question of fact, but whether such a situation contains the grounds for a decision of a particular kind is a logical question to be answered by analysis of the rules of procedure without any reference to facts *qua* actual and the time of their occurrence.

As soon as it is realized that the criteria of the correctness of a scientific decision refer to a given situation, the dilemma be-



tween infinite regress and 'ultimate grounds' disappears. The issue of ultimate grounds does not emerge at all.

As we have already mentioned, it is essential for empirical procedure that no proposition belonging to a science is exempt from the possibility of future elimination. This means that the system of rules of an empirical science must be so constructed that the elimination of any accepted proposition is rendered possible. We shall call this principle the *principle of permanent control*. Of course, this does not imply that every proposition belonging to science is continuously controlled, but only that no proposition belonging to science is exempt in principle from control. A system of rules that did not provide for permanent control would not be called a system of rules of procedure of an empirical science.

The reversibility of steps in empirical science is in contrast to the irreversibility of steps in deductive reasoning. If a proof of a mathematical proposition is correct, the theorem can never be invalidated. This is at the core of the distinction between absolutely certain rational knowledge and 'merely probable' empirical knowledge. No reference to different intensities of belief or to error in thinking is involved here. Errors may occur in either case, but that is not the issue. Failure to realize this has caused much confusion in discussions of the meaning of 'probability,' as we shall see in Chapter VII.

When a proposition that belongs to a science is eliminated, it may be replaced by a proposition incompatible with it. Thus, if a physical law is dropped because a prediction in terms of it does not withstand the test of observation, a singular proposition, namely, the statement of what is observed, is incorporated. But the elimination of a proposition need not be accompanied by the incorporation of a proposition incompatible with it. An accepted proposition may be eliminated because it has lost its foundations through the elimination of other related propositions, without being replaced by a proposition incompatible with it.

It follows that there are three different types of correct scientific decision in a science *S* concerning a given proposition *p*:

1.  $p$  is incorporated into  $S$ . We shall call this step the *verification* of  $p$ .
2.  $p$  is eliminated from  $S$  without being replaced by an incompatible proposition. This step will be called the *invalidation* of  $p$ .
3.  $p$  is eliminated and replaced by a proposition incompatible with it. Here we shall speak of the *falsification* of  $p$ .<sup>5</sup>

The falsification of  $p$  consists, then, of two steps, namely, the invalidation of  $p$  and the verification of a proposition incompatible with  $p$ . Invalidation without falsification has not been given sufficient attention in methodological analysis.

It should be noted that the elimination of propositions on which the verification of a proposition  $p$  was based need not have the elimination of  $p$  as a consequence. Other propositions may have been accepted in the meantime that rule out the elimination. Let us suppose, for instance, that the proposition 'There is blue at place  $p$  at time  $t$ ' was admitted into a science on the basis of the report of an observer (protocol proposition). Recognition of the protocol proposition may later be withdrawn because it is found that the observation occurred at a time other than that indicated. Thus, the proposition 'There is blue at place  $p$  at time  $t$ ' would have lost the ground by virtue of which it was originally accepted. But meanwhile a universal proposition may have been incorporated from which—together with an accepted singular proposition—our proposition can be deduced. Its elimination is then out of the question.

The preceding observations are open to the objection that there is insufficient precision in the underlying concept of scientific situation. Since the process of control is never definitely concluded—so it may be argued—and since it is much the same before and after acceptance, it is arbitrary to draw a sharp line of demarcation between accepted and unaccepted propositions.

This objection may be met by pointing out that the procedure of scientific control presupposes such a strict division. A proposition  $p$  is verified or falsified by showing that it agrees or dis-

agrees not with any arbitrarily chosen propositions, but with *accepted* propositions. In order to know what propositions can function in the control of  $p$ , we must be able to decide whether a given proposition is accepted or not.

We may say in summary that every complete question regarding the correctness of a scientific decision  $d$  has the following form: *Is  $d$  correct in terms of a system of rules  $R_1, R_2, \dots R_n$  in a given scientific situation?* If so, a proposition  $p$  verified by  $d$  may be called 'objectively valid.' It is therefore elliptical to speak simply of the objective validity of a proposition without indicating the rules of procedure in terms of which it is valid. Scientists who distinguish between objectively valid (warranted) assertions and not-objectively valid (unwarranted) assertions would usually be unable to complete the elliptical formulation, i.e. to state explicitly the rules that they implicitly presuppose. But the fact that there are different degrees of clarity in actual thinking is not relevant in logic. Both deductive logic in the strict sense and the logic of scientific procedure (methodology) refer to perfectly clear thinking. If a scientist is asked to substantiate his claim that an assertion is warranted, he must be able to point to other propositions as its grounds. But then he may be further asked, 'Why can these propositions be considered as grounds for your assertion?' This question, obviously, does not aim at a causal explanation, but at a clarification of the meaning of 'ground' (or 'warranted' or 'objective validity') and, accordingly, the answer consists in an explicit formulation of the presupposed rules of procedure. We shall have to refer to this point again in our discussion of preference rules of procedure in the next chapter, in our analysis of probability in Chapter VII, of value judgment in Chapters IX and XV, and of the objectivity of social science in Chapter XIV.

It should be noted in this context that the term 'objectivity' is used ambiguously. There is, in the first place, the distinction between objective sentences and sentences that contain subjective terms, such as 'I,' 'now,' 'here.'<sup>6</sup> The former, but not the latter, can be understood without reference to the speaker and his posi-

tion in space and time. But this distinction is merely linguistic. Every sentence that is not objective in the sense indicated can be translated into an objective sentence. No procedural rules are involved here.

Then we have the 'objectivity' just discussed, i.e. 'objective validity' of propositions in terms of presupposed procedural rules.

Finally, it may be asked whether the *rules* are 'objectively' given, and this question again may have two different meanings. It may refer either to the 'justification' of rules in terms of what we shall call 'rules of higher order' or to the consensus of scientists regarding the rules. But the question of actual consensus is not one of the logic of scientific procedure. In the natural sciences agreement on the basic rules of procedure extends so widely that their 'objectivity' is hardly questioned. But matters are different, or rather seem to be different, in the social sciences.

As we have mentioned, the propositions that are referred to as grounds in a scientific decision may belong to the science *S* under consideration, or to another science, which is then called an *auxiliary* science of *S*, or, finally, they may be reports of observations of particular persons (protocol propositions). Some remarks must be made about propositions of this type.

The problem of determining the meaning and significance of protocol propositions in scientific inquiry must not be confounded with the epistemological question of the ultimate source of empirical knowledge. The epistemological theory of sensationalism is chiefly responsible for the confusion of these different issues. According to it, our sensations are caused by the physical objects, and these sensations, kept free from all inferences, yield the 'hard data,' the indubitable elements of knowledge.

We shall discuss this point in greater detail in Chapter VII. In the present context it is sufficient to note that, whatever else the problem of the ultimate source of knowledge may be, it is certainly not a problem of the logical analysis of empirical procedure. If we say that the acceptance of a proposition is based on observations, we mean that its grounds are accepted statements about observations of particular persons. The conditions

for the acceptance of such protocol propositions are not at issue in a decision in which they are taken for granted. If the issue of their validity is raised, it can be shown that they are usually accepted if there is no particular reason to doubt the reliability of the observer.

However, we must briefly refer to an erroneous view concerning the relation between protocol propositions and the statements about the physical world based upon them. According to this view the latter are logically implied in the former. We shall give an example. The meaning of the protocol proposition 'N.N. saw at time  $t$  blue at place  $p$ ' is 'N.N. had at time  $t$  the perception "blue at place  $p$ ."' But it is frequently interpreted as meaning 'There was blue at place  $p$  at time  $t$ , and N.N. saw it.' Were the scientist really to understand protocol propositions in this manner, he would have to accept incompatible singular propositions about the physical world in recognizing conflicting protocol propositions. It is apparent that he does not do so, though he usually regards the acceptance of the protocol proposition as a sufficient reason for the acceptance of the corresponding 'objective' proposition. This holds not only for protocol propositions that refer to sense observations, but also for those that refer to self-observations or observations of the behavior of other persons. The interpretation here rejected is a further example of the confusion of deductive logic in the strict sense and the logic of empirical procedure, or, what amounts to the same thing, of the ambiguity of the term 'ground.'

The logical rules of empirical procedure are concerned with a given proposition  $p$  in two different ways. They determine (a) the control of  $p$ , and (b) its function in the control of other propositions. Accordingly, a step in  $S$  may relate to a proposition  $p$  in two ways. The step can, on the one hand, represent a change in the status of  $p$ —its acceptance into or elimination from  $S$ . It can, on the other hand, relate to  $p$  as one of the grounds for the change of status of another proposition.

An important division of the procedural rules of a given science  $S$  rests upon whether or not they contain a reference to protocol

propositions among the conditions determining a change in the corpus of S. This division is related to the logical form of propositions, more precisely, to the distinction between universal and singular propositions. There is, namely, no reference to protocol propositions in any rule relating to the *acceptance* of universal propositions. By saying that the incorporation of (new) universal propositions into a science must be based on facts, we mean that regard must be given to accepted singular propositions, but not to protocol propositions. The verification of those singular propositions in turn is frequently based on protocol propositions, but this cannot be gathered from the scientific situation to which the acceptance of the universal proposition is related. Lack of a clear distinction between protocol propositions and the propositions based upon them, together with failure to analyze procedure into single well-defined steps, is responsible for the insufficient attention devoted to this point. One alludes to it, however, in declaring, on the one hand, that assertions of fact rest on observation, and, on the other hand, that laws rest on facts.

Protocol propositions may be included among the grounds for the acceptance or elimination of singular propositions. But this need not be the case; and if it is, then the protocol propositions need not be the sole grounds. Universal propositions too are frequently found among these grounds, as is indicated in emphasizing that theory is implicit in the facts.

The untenable sensationalist view of a hierarchy of controls headed by the indubitable results of perception would lead us to believe that control works only from protocol propositions to universal propositions. As a matter of fact, however, it works the other way too: we may withdraw recognition from a protocol proposition because it cannot be brought into accord with an accepted synthetic universal proposition (empirical law).

Still, in a sense, protocol propositions are primary, since, no matter how well established synthetic universal propositions may be, they always face the possibility of being falsified by the results of observation; whereas the converse does not hold unrestrictedly. If further observations performed under very simi-

lar conditions agree with a protocol proposition the recognition of which was at first withdrawn, then it is the law conflicting with it that will finally have to yield. We can conceive of factual assertions so strongly supported by observations (protocol propositions) that no established theory alone can invalidate them.

There will almost always be universal propositions among the grounds for the acceptance of universal propositions—*inductio per enumerationem simplicem* plays scarcely any role in science—though they may often be lacking among the grounds of elimination.

We have already indicated one other difference between procedural rules for universal and singular propositions, namely, that there are cases in which singular propositions must be accepted into science whereas the acceptance of a universal proposition is never obligatory. But this difference does not apply to the elimination of propositions. The principle of permanent control requires that conditions for the obligatory elimination of universal as well as singular propositions be established.

Let us now consider the case in which the elimination of a proposition  $p$  is combined with the acceptance of a proposition incompatible with  $p$ . This is taken to mean that these steps are warranted by the same scientific situation. We shall speak here of *simultaneous steps* in scientific procedure, which, however, should not suggest the erroneous idea that a time factor enters into the *logic* of scientific procedure. This expression seems justified by the following considerations. Were we to describe such a process by saying that  $p$  is eliminated first and then non- $p$  (any proposition incompatible with  $p$ ) accepted, we should be leaving out of account the fact that the same situation warrants the elimination of  $p$  and the acceptance of non- $p$ . The same criticism is applicable to the statement that acceptance comes first and elimination follows. Furthermore, such an interpretation is untenable because it would imply that two incompatible propositions can simultaneously belong to a science. It follows from the above that protocol propositions may be among the grounds for the elimination of universal propositions. If we consider that

a proposition the incorporation of which into a science is at issue may be incompatible with any number of propositions belonging to that science, we see that there is no limit to the number of simultaneous steps, i.e. of steps warranted by the same scientific situation.

So far we have not referred to the principles of identity, contradiction, and the excluded middle—usually called the laws of thought. The principle of identity has no procedural correlate. It simply states that fixed meanings of terms and sentences are presupposed in scientific procedure. The case is different, however, for the principles of contradiction and the excluded middle. Before we analyze this point, we shall have to make some preliminary remarks concerning the meaning of 'truth' as related to synthetic propositions.

If we call propositions belonging to a science 'true,' this term must not be associated with the idea of unchangeability. According to the principle of permanent control, it is always conceivable that a proposition belonging to science at one time may be eliminated later and replaced by a proposition incompatible with it, i.e. 'falsified.' If the terms 'truth' and 'falsity' are used in this sense, we should have to say that a proposition true yesterday may be false today. Yet few logicians would agree to this formulation. Most logicians would rather say that the correct formulation is that the proposition was erroneously assumed to be true but later on was revealed as false. They would also insist that truth is independent of the knowledge of it and that a fact remains unaffected by our awareness or lack of awareness of it.

However, the rules of empirical procedure do not refer to ultimate truth or falsity, but to the processes of verification, invalidation, and falsification. This does not imply that 'verified' is the only meaning of the term 'true' that has methodological significance. What it does imply is that no meaning of the term 'truth' that is unrelated to the meaning of 'verification' can have any methodological significance. The first thesis identifies truth with *actual* knowledge; the second relates truth to *potential* knowledge. Failure to distinguish between them is largely re-



sponsible for the unceasing controversy over the nature of truth. We seem to be caught on the horns of a dilemma. There is, on the one hand, the idea of a transcendent truth unrelated to possible human experience, and therefore empty, and on the other, the idea of changing truth, which seems to be at variance with the fundamental principles of logic. But there is no such dilemma. The following analysis of the procedural correlates of the principles of contradiction and the excluded middle will help to clarify this point.

The traditional formulation of the principle of contradiction is that a proposition and its negation cannot both be true. This presupposes a structure of meanings that determines whether two given propositions contradict each other (are incompatible), i.e. whether each of them implies a negation of the other. Thus, for example, the two propositions 'There is red at time  $t$  at place  $p$ ' and 'There is blue at time  $t$  at place  $p$ ' are incompatible. This statement does not imply any reference to empirical procedure.

To determine the meaning of the correlate of this principle in empirical procedure we must replace 'true' by 'verified.' Then we may formulate it as follows: A scientific decision must not lead to a scientific situation containing two incompatible propositions. The following considerations will show that this principle must be interpreted as a property of the system of procedural rules rather than as a single procedural rule.

To interpret it as stating that no proposition should be accepted in a science if it is incompatible with another proposition belonging to that science would not be in accord with the rules of procedure actually applied, for these indeed provide for the replacement of propositions by others incompatible with them. But the procedural correlate of the principle of contradiction provides no clue to the conditions under which such a replacement can occur. If, as a result of a scientific decision, two incompatible propositions are declared to belong to a science, it may be stated that there is an error somewhere. But the procedural correlate of the principle of contradiction is of no aid in locating the error. It might lie either in accepting the one prop-

osition or in failing to eliminate the other. Thus we cannot judge in terms of this principle whether any given scientific decision is correct; hence the procedural correlate of the principle of contradiction must not be interpreted as a single rule of procedure. As already mentioned in the last chapter, it determines a property of the system of rules.

To exclude the emergence of contradictions, a system must fulfil the following conditions:

1. Simultaneous acceptance of incompatible propositions must not be permitted by the rules of procedure.
2. Simultaneously with the admission of a proposition into a science, the elimination of all propositions incompatible with it must be prescribed.

A system of procedural rules satisfying these two conditions may be called a *consistent system* of procedural rules. By saying that a system of rules of procedure must be consistent, we mean that otherwise it would not be called a system of rules of *scientific* procedure. This is tantamount to saying that science must be free from contradiction.

These analyses will appear familiar to logicians and mathematicians concerned with problems of postulational theory, particularly with meta-logic or meta-mathematics. These disciplines establish, in terms of the rules of the 'language' of a calculus (formation rules), what is meant by 'incompatibility of formulae,' and then examine whether the 'rules of procedure' of this calculus (transformation rules), prescribing the conditions of acceptance of new formulae, exclude the possibility of proving two 'incompatible' formulae.

Whereas the possibility that two incompatible propositions may belong to a scientific situation is ruled out by the procedural correlate of the principle of contradiction, the procedural correlate of the principle of the excluded middle states that neither of two incompatible propositions must be excluded from possible acceptance. If it is formulated as 'Either  $p$  or non- $p$  is true' and 'true' understood as 'verified,' then it is not in agree-

ment with scientific procedure. For there are undecided propositions at every stage of scientific inquiry.

What our principle does state is that no proposition must be undecidable in all conceivable situations. In other words, the system of rules of any science *S* must establish conditions for the verification of any proposition *p* belonging to the subject matter of *S*. (It is important to note that the meaning of 'decidable' or 'undecidable' is relative to a given system of rules of procedure.) If the system of procedural rules of *S* satisfies these conditions—in which case we shall call it a *complete* system of rules—every proposition *p* pertaining to the theme of *S* *can* be true in the sense of being verifiable in principle.

The requirement of completeness imposed upon a system of rules of scientific procedure means that a system of procedural rules can be called a system of rules of *scientific* procedure only if it satisfies this condition. Whether completeness is actually required today is a question that need not be discussed here. It has been maintained that the recognition of Heisenberg's principle of indeterminacy in quantum physics, according to which there is a finite limit to the precision possible in the simultaneous determination of two 'complementary magnitudes' (e.g. the position and impulse of an electron), means the abandonment by modern physics of the principle of the excluded middle. The appropriateness of this interpretation will be appraised by examining whether the questions that are undecidable according to Heisenberg's principle are meaningful questions, i.e. whether they can be formulated in an adequate language of physics.<sup>7</sup> But whatever the result of such an analysis, the very posing of the problem makes it clear that the procedural correlate of the principle of the excluded middle is a *principle of determinacy*. Failure to differentiate between deductive logic in the strict sense and the logic of scientific procedure is responsible for the continual confusion of the principles of contradiction and the excluded middle with their procedural correlates. It can be traced back to Aristotle, who discussed the question whether the principle of the excluded middle holds for unpredictable future events.<sup>8</sup> But

the principle of the excluded middle in deductive logic, stating that there is nothing 'between' two contradictories, is derived from the meaning of 'contradictories' and does not imply any reference to prediction or to scientific procedure in general.

The procedural correlate of the principle of the excluded middle is a kind of counterpart of the principle of permanent control. Whereas the former states that the rules of every science must render possible the acceptance of any proposition pertaining to its theme, the latter states that the possibility of eliminating any proposition belonging to a science must be established. Like the procedural correlates of the principles of contradiction and the excluded middle, this principle is not a rule of procedure but determines *a property of the system* of procedural rules.

It follows from our analysis of the procedural correlate of the principle of the excluded middle that when we seek to replace the concept of transcendent truth by one that is relevant for scientific procedure, the concept 'verified' is proper only in some cases. In others, we must replace 'true' by 'verifiable,' thus relating the meaning of this term to potential rather than actual procedure—and moreover to a potentially endless procedure. Then we may define a 'true synthetic proposition' as one that could be accepted if we had all the knowledge relevant (in terms of the rules of procedure) for the scientific decision concerning its acceptance and that, once accepted, could withstand all possible controls.

A goal defined in terms of a potentially endless procedure may be called an *ideal*, and accordingly the incorporation of permanently valid propositions may be termed an ideal of inquiry. In this sense we may speak of the ideal of truth. It is the scientist's desire to incorporate propositions that could survive all possible controls. The belief that such propositions can be found is 'the belief in truth.' It can be neither proved nor refuted because we cannot go through an endless process.

The ideal of truth is related to other ideals of inquiry. For scientists do not wish to incorporate random propositions able to withstand all possible controls; rather, they aim at establishing

a system of laws permitting the solution of every conceivable problem. We shall have more to say in the next chapter regarding this comprehensive theoretical ideal and its relation to the rules of scientific procedure.

'Truth of synthetic propositions,' understood as an ideal of inquiry, is defined in terms of possible human experience, though we can never definitely establish that we have attained truth.<sup>9</sup> It is otherwise with the notion of truth-in-itself or absolute truth understood as an idea in the Infinite Mind of God, whose knowledge is exclusively rational knowledge. Since rational knowledge and empirical knowledge are different in kind, there is no way that leads from the conception of absolute truth to a genuine logical theory of empirical procedure, i.e. *a theory of correct scientific decisions in given scientific situations*.

As we have already pointed out, one fundamental difference between deductive logic in the strict sense and the logic of empirical procedure is that the rules of deductive inference are derivable from the propositional meanings and are thus 'ultimately rational,' whereas the rules of empirical procedure are not. On the other hand, it should not be overlooked that, the rules of empirical procedure and a scientific situation being given, the correctness of scientific decisions is provable by pure reason. In this respect the logic of empirical procedure is not 'less rational' than deductive logic in the strict sense. Otherwise, it would be inappropriate to speak of a *logic* of empirical procedure.

These considerations lead to an appraisal of skepticism. If the principle of skepticism is taken to be the thesis that the validity of a synthetic proposition can never be regarded as ultimately established, then we can derive the skeptical doctrine from the meaning of scientific procedure with its principle of permanent control; it is a result of the clarification of scientific procedure. But it cannot be proved, either, that no proposition is able to withstand all possible controls. If, then, skepticism is taken to assert that the belief in truth is inconsistent, skepticism is wrong. Furthermore, it is wrong if applied to logic and pure mathematics, i.e. to analytic propositions. It is essential for an ap-

praisal of the issue of skepticism to realize that it is at bottom the issue of the meaning of 'truth.'

When it is seen that the meaning of this term, if applied to analytic propositions, is essentially different from its meaning when applied to synthetic propositions, truth in the former sense will no longer be regarded as a prototype for truth understood in the latter sense.

To avoid the difficulties arising from the ambiguity of the term 'truth' as far as synthetic propositions are concerned, we shall call a synthetic proposition incorporated in a science, 'empirically valid,' and all propositions incompatible with a valid proposition 'empirically counter-valid.' Propositions that are neither empirically valid nor empirically counter-valid will be called 'undecided.'

IN approaching the problems to be dealt with in this chapter, we shall first differentiate between theoretical and non-theoretical goals of scientific inquiry.

To promote the welfare of humanity by the results of his investigations may be the ultimate aim of one scientist. Another may set as his goal the achievement of material security and social prestige for himself. A third may find scientific inquiry 'an end in itself,' in the sense that the satisfaction he derives from his scientific work is reason enough for his engaging in it. In all these cases scientific activity appears as a means to the attainment of ends that are not defined exclusively in terms of the scientific process. There are, however, proximate goals of every scientist *qua* scientist, namely, to answer pertinent questions. We shall call these goals *theoretical goals* of scientific inquiry. *They are defined exclusively in terms of deductive logic and empirical procedure.*

Our next task is to clarify the meaning of 'problem' and 'solution of a problem.' To pose a problem is to set the theoretical goal of giving a correct answer to a question in science. We shall first distinguish between logical problems and empirical problems. To solve a logical problem is to explicate an implicit meaning. To solve an empirical problem is to perform a series of steps terminating in the verification of an answer to a question of fact.

If the question can be answered by 'yes' or 'no,' the problem is solved by the verification of either of two contradictory

propositions. Thus, the problem 'Has water a density greater at 4° C than at 3° C?' is solved by verifying the proposition 'The density of water at 4° C is greater than its density at 3° C.' If we ask on the other hand, 'What is the speed of light in empty space?' the correct answer does not consist in the verification of one of two contradictory propositions, but in the verification of a proposition obtained from the propositional function 'x is the speed of light in empty space,' by substituting for the variable 'x' a constant denoting a definite magnitude. It is then seen that the first class of problems may be subsumed under the larger class of those having only a limited number of conceivable solutions. Thus, the question, 'Which of the 21 American republics has the smallest area?' has precisely 21 possible solutions if we exclude the possibility that two or more of those areas may be exactly equal. Accordingly, we have to differentiate between problems aiming at selections among a *limited* number of propositions and problems aiming at selections among an *unlimited* number of propositions. The underlying difference between the two types of question and the *possible* answers to them is given by their meanings; it is not related to scientific procedure, which aims at *correct* answers. The term 'answer' is ambiguous, and this ambiguity reflects the confusion between issues exclusively relating to propositional meanings and issues of verification, i.e. of empirical procedure. Possible answers to a given question are exclusively determined by the meaning of the question. The distinction between 'correct answers' and 'incorrect answers,' however, is in terms of the rules of procedure.

These considerations lead to a distinction between logical problems and empirical problems of 'explanation' and 'prediction.' Reference to facts and laws is required in the explanation of facts. Explanation in terms of laws understood as synthetic universal propositions simply makes it explicit that the given science already contains the proposition in question, that it can be deduced from its corpus at the time the question is posed.

As we shall point out in Chapter VI, the case is different when the law referred to is not a synthetic universal proposition, but,



for the present, we shall not consider this type of law. Explanation, then, does not involve any scientific decision if both law and fact (major premise and minor premise) are established.

However, scientific decisions are required if one or both of the premises are yet to be 'discovered,' i.e. verified.

Accordingly, four different situations with respect to the explanation of facts may be distinguished:

1. The explanation may be logically entailed in already established knowledge.
2. The laws referred to in the explanation may have been established, but not the facts required.
3. The facts referred to in the explanation may have been established, but not the laws.
4. Neither the laws nor the facts required may have been established.

This classification applies to predictions as well, since the logical structure of explanation is identical with that of prediction.<sup>1</sup> A fact could have been predicted on the basis of the same propositions that explain it.

The establishment of a correct answer to a question may require a long chain of steps, and a scientist shows his acumen in envisaging such a chain. To call a correct answer to a given question a solution of a problem is not quite unambiguous. It may mean either that the answer, if correct, is the solution, or that the solution consists in the procedure by which the correctness of the answer is proved. The latter interpretation is preferable in a logical analysis of the rules of procedure because it refers to the criteria of correctness. It is thus more proper to call the *chain* of steps leading from the initial situation to the answer a solution of the problem. We say *a* solution, not *the* solution, because a problem may have several solutions.

Every subset of a set of steps that is a solution of a scientific problem may be called *relevant* to this solution, but, used in this sense, the term 'relevance' should not be interpreted teleologically. The subsets under consideration are not means to, but

parts of, the solution. We shall offer an analogy. Boarding a Chicago express in New York at noon is a means to the end of being in Chicago the next morning, but it should not be said that the journey from New York to Pittsburgh is a *means* to traveling from New York to Chicago. It is a *part* of the trip—one of various possible trips—from New York to Chicago.

If a certain chain of steps is a solution of a problem, then it, and each step in it, is by definition relevant to this solution. But this yields no criterion for determining the direction the inquiry should take to solve the problem. It cannot justify any choice between alternative approaches to an unsolved problem. Yet it may be possible to justify such preferences, and in doing so, we presuppose rules of preference. The following remarks will elucidate this point.

Few, if any, scientific problems are so 'new' that we have no indication whatever concerning the path toward their solution. In many cases, particularly in the natural sciences, there are well-defined patterns for the solution of problems, which become, accordingly, a matter of routine. This is the case when events are predicted in terms of well-established laws and it is only required that observations of a certain type be made in order to obtain the pertinent data. Often the task is more difficult, but patterns can still be recognized, as for example in the explanation of infectious disease in terms of effects brought about by microbes or viruses. Today, a scientist would be criticized as having chosen an inappropriate approach toward solving the problem of explaining the origin of infantile paralysis if he collected statistical data concerning the relation between variations of temperature and the incidence of this disease because he wanted material in support of the hypothesis that infantile paralysis is caused by fluctuations in temperature. To be sure, by doing so, the scientist has not violated the basic procedural rules of his science. He has not yet made the unfounded assertion that infantile paralysis is produced by temperature fluctuations, but has merely suggested that it is worth while to examine this hypothesis by collecting pertinent statistical material. However,

at the present state of knowledge, it is generally believed that such data would be of no avail for the explanation of epidemics of infantile paralysis. Accordingly, we can say that the gathering of data concerning temperature variations is *presumably irrelevant* to the explanation of the origin of infantile paralysis.

For any given problem we can distinguish between chains of steps that are presumably relevant and chains of steps that are presumably irrelevant to its solution. The criteria of presumable relevance are preference rules of scientific procedure. A scientist who rejects a certain approach to the solution of a given problem as inappropriate is bound to give his reasons, and, in doing so, he has to make explicit the implicitly presupposed preference rules. What we have said in preceding chapters about the givenness of rules of procedure is applicable here. All objective criticism, whether it judges the acceptance of an assertion as unwarranted or an attempt toward the solution of a given problem as presumably irrelevant, refers to given rules that are declared to have been violated in the case under consideration. In other words, the criticism is in terms of these rules, just as the criticism of a mode of speech as ungrammatical is in terms of presupposed rules of grammar. One cannot substantiate either type of criticism without making explicit the underlying rules. This argument cannot be refuted by emphasizing that there may be lack of consensus concerning procedural rules and that they are not invariable. What is at issue is not reference to one rule rather than another, but the relativity of methodological criticism (as of any other criticism) to presupposed rules.

We have to note that preference rules have a relational structure of higher complexity than some basic rules, since they implicitly refer to basic rules. The selection in terms of preference rules is between steps or chains of steps, that is, between scientific decisions in conformity with the basic rules. Moreover, the terms 'problems' and 'solutions of problems' that are referred to in the definition of 'preference rules' are also defined in terms of basic rules, as we have emphasized at the beginning of this chapter.

There are two major varieties of preference rules in scientific procedure: (1) those that concern the preferability of one procedure over another in the approach to a given problem, taking the problems as given, and (2) those that concern the relative importance of problems and of the procedures presumably relevant for their solution in terms of given ideals of science.

In discussing the concept of truth in the preceding chapter, we have said that it is an ideal of inquiry to incorporate into science 'true' propositions, i.e. propositions able to withstand any possible control. And in general we mean by 'ideal' a goal defined in terms of a potentially endless process.

Now a study of the history of philosophy and science discloses certain other ideals clustering about this ideal of truth. They were combined in the conception of a perfectly rational cosmos, planned and created by the Infinite Mind. It is the aim of science to discover the laws of this cosmos. We shall give a brief sketch of the main features of this world-view as it appears among scientists of the eighteenth and nineteenth centuries, under the influence of Newtonian physics, listing the chief theoretical ideals.

#### *a. The Ideal of Unity and Simplicity*

All laws are to be arranged in a hierarchy, at the head of which are a smaller number of principles (at best, a single principle). These laws assert mathematical relations among observable magnitudes. The more special laws are derived from the more general by decreasing abstraction, the scope of indeterminateness being restricted by the insertion of data. The number of independent variables is small and their mathematical relations are simple. The continuity and differentiability of mathematical functions are viewed as essential features of their simplicity. (The former is a necessary but—as Weierstrass proved—not a sufficient condition of the latter.) By the postulate of continuity, arbitrarily small changes in the dependent variables must correspond to sufficiently small changes in the independent variables. In fact,

differential equations of the second order dominate classical physics in Newton's mechanics as well as in Maxwell's electromagnetic theory of light.<sup>2</sup>

*b. The Ideal of Unrestricted Universality*

The highest laws of the system are unrestrictedly universal. They are not restricted to our solar system or to a few billion years before or after our era.<sup>3</sup> Certain more specific laws are subject to variations, but these are to be explained in terms of the general principles by variations in data.

*c. The Ideal of Precision*

A proposition is the more precise the more it restricts the frame of possibilities. Perfect precision is guaranteed by the fact that laws establish metrical relations among the facts to which they refer. All genuine laws, therefore, have mathematical form. Science, accordingly, is given the task of replacing qualitative by quantitative thinking.

*d. The Ideal of the Pervasiveness of Law*

There is no class of facts not falling under laws, no class of indeterminable factors. If all the data at any momentary state of the world were known, every fact of the past or future could be determined in terms of preëstablished laws. Laplace has given us the most striking formulation of the deterministic view.<sup>4</sup>

In the second part of the nineteenth century, physics seemed indeed to have reached a state satisfying these requirements. Since then, however (as we shall see in the next chapter), there have been significant changes in the interpretation of the laws of nature. It has come to be recognized that at the present stage of inquiry we cannot expect as pervasive a harmony among the goals of inquiry as was earlier assumed to exist.

The preference rules derived from the different ideals must be adjusted to each other as the scientific situation varies. Some ideals may have to recede into the background if we want to

comply with other ideals. This holds to an even greater extent in the social sciences than in the natural sciences. Here we can speak of a conflict among goals of inquiry, as we speak of a conflict among practical goals.

Many methodological controversies, particularly in psychology and the social sciences, are largely due to the misconception that there is a preëstablished harmony among the various ideals of inquiry and a single adequate approach to each given problem. Consequently, a methodological conflict is viewed as a difference of opinion about what is *the* appropriate method, whereas it would be more pertinent to compare the different problems and the merits of various methods separately with respect to each of the various ideals. Such a comparison may in certain circumstances lead to the recognition that one method is plainly preferable to another because it makes possible the solution not only of all problems soluble by the other, but of additional problems as well. Thus, during the nineteenth century Huygens' wave theory was granted undisputed superiority over the Newtonian corpuscular theory of light.<sup>5</sup> Very often, however, different methods will coexist within a certain field of inquiry, each leading to certain achievements denied to the others. The idea that for a given field of inquiry one method is exclusively appropriate is less harmful in physics, where the actual superiority of certain research methods has been clearly displayed, than in psychology and the social sciences. It seems that we shall have to resign ourselves for some time to methodological pluralism in these sciences.

We have seen that the attainment of an ideal can never be ultimately established. What we can do is to establish that the conditions in terms of which an ideal is defined are fulfilled at a particular time. Nevertheless, we may say, without danger of being misunderstood, that the ideal is fulfilled (or approached) if these conditions are fulfilled (or approached) in a given situation. Modern science has a higher degree of perfection in terms of these ideals than had earlier science, and this has strengthened

our belief that future knowledge will surpass present knowledge. But there is no 'ultimate' justification of this belief and no reference to it in the preference rules.

Hence methodological analysis of these rules does not refer to this belief though it deals with the rules of procedure, the observation of which has been motivated by the belief. Such analysis is concerned with clarifying these rules, i.e. with explicating the criteria by which we judge of the greater or lesser importance of a problem or method. It would be a serious error to dismiss the issues centering around preference rules on the ground that the sole touchstone of methods is their success. For the problem is precisely what is meant by 'success' or 'different degrees of success' and what criteria are used to judge the chances of success of a method in a given situation of inquiry.

Scientific ideals have been interpreted in three different ways. According to the first, the aprioristic interpretation, it is possible to reach the ideals, i.e. to establish ultimately the truth of synthetic propositions that fulfil the conditions required by these ideals, or the exclusive appropriateness of a particular method in a particular field. This interpretation is incompatible with the principle of permanent control.

The second interpretation is not incompatible with the principle of permanent control, but it gives the principle a subjective turn. The reason why we may have to eliminate previously accepted propositions is to be found in the fallibility of the human mind. Every proposition, however, is true or false in itself, and a mind exempt from the limitations of finitude could dispense with the principle of permanent control. Thus, this interpretation agrees with the first in holding that the ideals are in principle attainable, but it differs by denying that the human mind can ever be sure of having attained them. The chief objection to it is that it has no procedural significance.

The third interpretation views ideals as regulative principles of inquiry (in the Kantian sense) without ascribing to them a preëstablished transcendent existence. This interpretation is the

adequate one. The procedural significance of ideals is constituted by their relation to preference rules.

A clear understanding of the meaning and significance of theoretical ideals is hardly possible without a more thorough analysis of the nature of scientific laws. The following chapter will be devoted to such an analysis.



METHODOLOGICAL analysis of the nature of physical laws has been seriously hampered by the fact that it has been couched in such ambiguous terms as 'objectivity' and 'subjectivity,' 'real' and 'ideal.' The question has been asked whether the laws of nature have 'objective' existence (theologically interpreted as existence in the Mind of God) or merely 'subjective' existence (in the mind of man); whether they are real bonds between things or merely theoretical constructs. More recently the alternative has been expressed in the form: 'Are the laws discovered or invented?'<sup>1</sup>

The genuine methodological questions are:

- (a) What is the logical form of laws?
- (b) Under what conditions are such propositions accepted?
- (c) Under what conditions are such propositions eliminated?
- (d) What functions do such propositions have in the control of other propositions?
- (e) How far does the system of laws established at present fulfil the conditions implied in the ideal of a rational cosmos?

The chief point to be made in this chapter is that we have to differentiate between two types of laws, namely, synthetic universal propositions and rules of empirical procedure in the strict sense. This distinction will shed some light upon the questions listed above.

At the outset we must observe that for want of a clear dis-

inction between propositional meanings and rules for the verification of propositions, the terms 'fact' and 'law' are used ambiguously. When we say that a certain fact obtains or does not obtain or that a certain law holds or does not hold, we mean by 'fact' or 'law' the proposition, regardless of whether it is accepted, for it is precisely its acceptance that is at issue. If, on the other hand, we speak of the facts or laws of a certain science, we refer exclusively to accepted propositions.

For reasons that will soon become apparent we shall first refer to question (b). The traditional answer to this question is that laws are either deduced from more general laws or established by induction from observed facts. Now it follows from our analysis in the preceding chapters that these two cases are essentially different. There we pointed out that by deducing a proposition from other propositions already belonging to a science, we do not introduce a logically new proposition. No restrictions of the frame of possibilities are added. Induction, on the other hand, is understood as yielding laws that are new in this sense. But the relation between deduction and induction is still a controversial matter among methodologists, and the Whately-Mill doctrine of the 'ground of induction' still exercises a noticeable, though gradually decreasing, influence. A brief critical examination of this doctrine will lead us to the core of our argument.

Following Archbishop Whately,<sup>2</sup> Mill declared that every induction may be thrown into the form of a syllogism by supplying a major premise, and that this procedure leads to an ultimate syllogism the major premise of which is the *principle of the uniformity of the course of nature*. This principle asserts that 'there are such things in nature as parallel cases; that what happens once, will, under a sufficient degree of similarity of circumstances, happen again, and not only again, but as often as the same circumstances recur.' We shall quote the paragraph in which Mill undertakes to justify the introduction of this principle.

The statement, that the uniformity of the course of nature is the ultimate major premise in all cases of induction, may be

thought to require some explanation. The immediate major premise in every inductive argument, it certainly is not. Of that, Archbishop Whately's must be held to be the correct account. The induction, 'John, Peter, etc., are mortal, therefore all mankind are mortal,' may, as he justly says, be thrown into a syllogism by prefixing as a major premise (what is at any rate a necessary condition of the validity of the argument), namely, that what is true of John, Peter, etc., is true of all mankind. But how came we by this major premise? It is not self-evident; nay, in all cases of unwarranted generalization, it is not true. How, then, is it arrived at? Necessarily either by induction or ratiocination; and if by induction, the process, like all other inductive arguments, may be thrown into the form of a syllogism. This previous syllogism it is, therefore, necessary to construct. There is, in the long run, only one possible construction. The real proof that what is true of John, Peter, etc., is true of all mankind, can only be, that a different supposition would be inconsistent with the uniformity which we know to exist in the course of nature. Whether there would be this inconsistency or not, may be a matter of long and delicate inquiry; but unless there would, we have no sufficient ground for the major of the inductive syllogism. It hence appears, that if we throw the whole course of any inductive argument into a series of syllogisms, we shall arrive by more or fewer steps at an ultimate syllogism, which will have for its major premise the principle, or axiom, of the uniformity of the course of nature.<sup>3</sup>

But it could not long remain unnoticed that this principle does not fulfil the function ascribed to it by Mill, that of forming the basis of inductive inference.<sup>4</sup> Two important strictures may be quoted from the text by Cohen and Nagel.<sup>5</sup>

The principle is stated in an extremely vague form—'what happened once, will, under a sufficient degree of similarity of circumstances, happen again.' But what is a sufficient degree of similarity? The principle does not tell us. In any particular investigation we must rely on other criteria, if there are any, to determine what are the circumstances material to the occurrence of a phenomenon . . .

. . . the principle does not affirm that *every* pair of phenomena are invariably related. It simply states that *some* pairs are so connected. To appeal to the doctrine in a particular investigation is therefore useless.

That Mill, who has so vigorously insisted on the primacy of induction over deduction, should have endorsed Whately's view may be explained as a residue of the idea, deeply rooted among logicians, that all inference in the empirical sciences consists in deducing synthetic propositions from other synthetic propositions. In fact, however, inductive inference consists in subsumption under definitions—rules of procedure. Deductive inference shows that a synthetic proposition is implied by other synthetic propositions. Inductive inference shows that the acceptance of a proposition is *correct* in terms of presupposed rules. Thus, what is derived is not the proposition itself—for the conclusion of a syllogism the major premise of which is a definition cannot be a synthetic proposition—but rather the correctness of its acceptance into science.

In deciding whether the inference from 'John, Peter, etc. are mortal' to 'all men are mortal' is correct, we need not ask, as Whately and Mill do, whether there is a law of nature, understood as a synthetic universal proposition, of the form 'what is true of John, Peter, etc., is true of all mankind.' Rather, we have to make explicit the presupposed rules of scientific procedure in order to determine whether the inference under consideration is covered by them. Broadly speaking, a synthetic universal proposition states, 'No *p* without *q*,' while a rule of procedure declares, 'If *p* is known, acceptance of *q* is warranted.' A rule of procedure, but not a synthetic universal proposition, thus refers to the state of knowledge in a given situation. A synthetic universal proposition, but not a rule of procedure, can be *falsified*, though, under certain conditions, stated by rules of a higher order, a rule of procedure may have to be replaced by another. With regard to the example offered by Mill, we must accordingly say that the inference from 'John, Peter, etc., are mortal' to 'All

men are mortal' is not derivable from any ultimate ground by a chain of syllogisms.

Whately's and Mill's attempt to find an 'ultimate ground' for induction is closely related to the theories of immediate knowledge according to which all knowledge must be derivable from self-evident truths. No one has shown more clearly than John Dewey<sup>6</sup> that we here come upon a fundamental error, common to both rationalist and empiricist doctrines. The rationalists hold that ultimate principles of universal character are the object of immediate knowledge; the empiricists, that what is immediately known are sensory qualities or sense data. Dewey makes it clear that none of these theories can withstand critical examination. He who presupposes that all knowledge is either immediate or logically derived from immediate knowledge is bound to misinterpret empirical procedure. Only understanding of meanings can be called 'self-evident immediate knowledge.'

It may be objected that the foregoing criticism of the principle of the uniformity of nature goes too far if it denies the significance of the principle for scientific inquiry. For the assumption that under approximately equal conditions events of the same kind will occur is taken for granted in empirical science.

To this we must reply that laws are not *based* on the principle of the uniformity of nature but that they are themselves statements of uniformities. Laws may be of different degrees of generality, but it is wrong to regard the principle of the uniformity of nature as the most general law. It does not restrict the frame of possibilities in any definite way, since it does not tell us wherein the uniformity consists. We have here an instance of an error that is often encountered in the history of philosophy, viz. the confusion between propositional forms and synthetic propositions of highest generality. In the endeavor to ascend to ever more embracing universal propositions, we are in danger of landing in the void. Like the Tower of Babel, the logical pyramid collapses from 'confusion of tongues' as soon as it spurns limitation.

As for the *belief* in the uniformity of nature, the remarks made

in Chapter IV concerning the belief in truth are applicable here. The belief in truth, we have suggested, is the belief that propositions can be discovered capable of withstanding every possible control. The belief in the uniformity of nature is the belief that *universal* propositions of such a kind can be found. This belief is strengthened by the discovery of universal propositions that withstand many controls, but it cannot be *proved* that a proposition which has withstood many controls will survive all further controls. The causes of the occurrence of a belief must be sharply distinguished from the criteria of its correctness.

The controls that a universal proposition must withstand in order to be accepted into science are established by the rules of procedure of science. Mill's theory of induction can be viewed as an attempt to make these rules explicit; but notwithstanding his considerable advance beyond Bacon, he cannot be said to have been successful. Nevertheless, he strongly influenced even those modern logicians who recognized his shortcomings. Most of these shortcomings are due to sensationalist epistemology, according to which isolated sense data form the building blocks of knowledge, and laws are viewed as collections of particulars. This view is the chief root of Mill's thesis that 'all inference is from particulars to particulars.' One may reject this thesis without accepting the antithesis, 'No inference is from particulars to particulars.'

Thorough analysis of this issue leads to the core of the logical problems relating to the control of laws and predictions. The usual interpretation of the incorporation of singular propositions is that they must be either accepted on the basis of observations or deduced from universal propositions in combination with singular propositions. By deducing a proposition from other synthetic propositions already belonging to a science, we do not incorporate a new proposition. We only make clear that this proposition has already been verified. Many predictions, however, that are usually supposed to have been deduced from accepted synthetic propositions, were actually verified by an empirical procedure.

To prove this, we shall have to show that many propositions traditionally interpreted as synthetic universal propositions are specific rules of empirical procedure. A synthetic universal proposition is falsified by a single negative instance. Frequently, however, the non-fulfilment of a warranted prediction does not result in the elimination of any universal proposition. To declare in such cases that the prediction has been derived from a synthetic universal proposition would involve inconsistency. To show the stringency of this argument, we must briefly examine some of the interpretations offered in such instances.

One interpretation emphasizes that a universal proposition in a science should not be tested in isolation but only within the context of the theory of which it is a part. Thus, if the results of observation seem to falsify a given law, it may be that not this law, but another law within the frame of a theory from which the prediction has been derived ought to be eliminated. However, this argument does not touch the root of the problem. An observational test may be understood as a control of a set of universal propositions regarded as a unity, or of a single universal proposition. If the test is taken to be a control of a single universal proposition, all other propositions that constitute the theory are taken for granted as far as this test is concerned. The meaning of a test is not understood unless we determine unambiguously what should be tested.

Not infrequently the distinction is made between *strict laws* that are falsified by a single negative instance and other synthetic universal propositions—often called ‘mere rules’—that allow for exceptions. But this distinction is not acceptable. To declare that a science may contain synthetic universal propositions allowing for exceptions is tantamount to admitting that it may contain contradictory propositions, an admission hardly intended by those making this point. It may be objected that a rule should be understood as referring not to all of a kind but to most of a kind. But this interpretation would not remove the difficulty. Statements of the form ‘For most  $x$  and  $y$ ,  $x$  implies  $y$ ’ are not empirically testable if they are meant to include all the

$x$ 's and  $y$ 's in the universe. The universe as a whole is not given by experience, and there is no sample that can be regarded as representative of the distribution in the universe. The term 'most' has no empirical significance unless related to a finite number of objects.

Finally, reference is sometimes made to 'disturbing factors,' with the qualification that a law should always be taken to imply a *ceteris paribus* clause. If the prediction derived from the universal proposition to be tested is not fulfilled, then it may be declared that 'the other things' were not equal. This argument, however, is of no avail. A law understood as a synthetic universal proposition asserts that events of a kind  $e_2$  occur without any exception, if events of the kind  $e_1$  occur. It asserts invariances with respect to all factors not referred to in the law itself. To add any conditions for its validity is to declare that it is not valid as it stands.

The *ceteris paribus* clause can be understood in two different ways. The *cetera* may consist of only a definite number of well-determined factors. Then we still have a synthetic universal proposition, though less general than the original one. Or, the factors to be subsumed under the clause may be left partly or completely undetermined. No possibility is excluded by a sentence to which such a clause is added, because we can always make an unknown factor responsible for the non-fulfilment of predictions made in terms of the law. This implies that the sentence is not understood as a synthetic proposition. We shall see, however, that it is not devoid of procedural significance.

Consequently, unless we grant the possibility that a scientific situation can contain contradictory propositions, we must admit that warranted predictions need not be deduced from synthetic universal propositions. For example, there may be a rule of procedure like this: 'If events of the kind  $e_2$  have ten times been found to succeed events of the kind  $e_1$  at a certain spatio-temporal distance and no negative instance has been observed, then it will be correct to predict that an event  $e_2$  will succeed an observed event  $e_1$  at the same spatio-temporal distance.' In



most instances the rules of procedure relating to predictions will be much more complex; the essential point, however, is that, in order to be regarded as warranted, the predictions need not be deduced from synthetic universal propositions.

It should be noted that a prediction warranted in terms of rules of procedure need not be fulfilled. We may distinguish four different possibilities:

1. Warranted predictions that are fulfilled.
2. Warranted predictions that are not fulfilled.
3. Unwarranted predictions that are fulfilled.
4. Unwarranted predictions that are not fulfilled.

We shall illustrate the preceding analysis by an example from physics, namely, Galileo's law of falling bodies. Considering the status of this law, the logician may ask, 'How can it be said that it is valid although empirical tests show in many cases that bodies do not behave in accordance with it?' The reply will usually be: 'The law holds only under certain conditions, e.g. in empty space.'

But the logician will not be satisfied with this interpretation. He will declare: It is of the very nature of a law to state an invariance with respect to all factors not referred to in the law itself. This leads us to the realization that the so-called empirical testing of physical laws of the type of Galileo's law of falling bodies is in fact a testing of more restricted laws. The restrictions are implied in the additional conditions that the experimental physicist considers as relevant for his test.

But even a negative result of the test of the restricted law would not necessarily mean that the more general law loses all significance for scientific inquiry. It may still be accepted as a rule of empirical procedure in terms of which warranted predictions of physical events are made. Such a rule may have to be replaced by another if it does not lead to the desired results, and accordingly it may be affected by the negative result of an empirical test. But it cannot be falsified. The *ceteris paribus* clause expresses the resolution to retain such procedural rules, under certain conditions, even if predictions made in terms of

them are not successful; and herein lies its procedural significance.

The crucial methodological problem in this context is thus: How can it be decided whether a law is a synthetic universal proposition or a rule of procedure? The answer is: It must be made clear whether the proposition is considered as falsifiable by an observational test (a negative instance). If it is regarded as falsifiable, then it is understood as a synthetic universal proposition; if not, it is understood as a rule of procedure. Of course, this does not mean that the difference between synthetic universal propositions and these procedural rules is constituted by procedural rules of falsification; the difference is one of propositional meaning. However, reference to the rules of falsification can be of aid in disclosing whether or not a given proposition is synthetic.

The usual way of pointing to this difference is to emphasize that the more general laws of physics are not meant to describe real cases with perfect precision but that they are concerned with ideal cases. It would lead us too far afield to study the historical relations of this interpretation to Plato's doctrine and its transformation in Aristotelian and Neo-Platonic teachings, but we may mention that Galileo was strongly influenced by it.

In analyzing Galileo's 'resolutive' and 'compositive' methods, Ernst Cassirer has aptly stated the methodological significance of 'idealization' in his thought.

For him . . . the particular is not primarily an isolated datum, but it is always determinable only as an intersection of universal relations. The Galilean equations of falling bodies do not claim to be assertions how bodies 'really' fall, for the very reason that they do not refer to the factor of the resistance of the air. But Galileo does not see in this a defect of his scheme; he sees in it only the requirement to proceed a step farther in determination and to represent the factor of resistance of the air by new rules which are also exact.<sup>7</sup>

Many modern philosophers of science who were much farther away from Platonism than was Galileo have discussed the mean-

ing and stressed the significance of idealization in physical laws. Ernst Mach, for example, writes,

All universal physical concepts and laws . . . the concepts of rays, the dioptric laws, the law of Mariotte, etc., are arrived at by idealization. They thereby assume that simple and also general, little determined form which makes it possible to reconstruct any facts, however complicated, by synthetic combination of these concepts and laws, thus making it possible to understand them.<sup>8</sup>

These philosophers were more influenced by Kant's conception of the ideal as a regulative principle of inquiry than by the Platonic conception. This holds particularly for the pragmatist approach to the problem.

All these analyses, however, do not sufficiently clarify the meaning of laws that allow for exceptions. The expression 'idealization' seems to suggest that these laws are synthetic universal propositions describing properties of an imagined rational cosmos. This is an inadequate interpretation. Such laws are not descriptions of an ideal cosmos, but prescriptions for scientific procedure concerned with the actual cosmos. In other words, they are not synthetic propositions, but rules of procedure. We shall call them *theoretical laws* as contrasted with *empirical laws*, which are synthetic universal propositions accepted in science.<sup>9</sup>

The chief methodological problems relating to theoretical laws are: (a) Under what conditions is the acceptance of a theoretical law warranted? (b) How are theoretical laws affected by the invalidation or falsification of correlated empirical laws?

These problems are at the core of the controversy between conventionalists and empiricists (in the strict sense). The declaration of the empiricists that only 'experience,' i.e. observation, can decide the validity of physical laws is appropriate as a rejection of the claim that there are necessarily valid statements about reality (synthetic propositions *a priori*). But it does not solve the problem of determining the status of theoretical laws

in physical inquiry. The conventionalists, on the other hand, who stressed the fact that theoretical laws cannot be falsified, were prone to underrate the significance of empirical testing, which is the backbone of empirical inquiry. But it cannot be justly maintained that empirical testing is completely disregarded by the conventionalists. They would not deny that in many cases the outcome of such tests bears on the decision whether a given convention should be accepted, or once accepted, should be upheld. In contrasting empirical laws with theoretical laws (conventions), we may say that empirical laws (in our terminology) can be considered 'more empirical' in the sense that the observational test—usually regarded as the distinctive feature of empirical procedure—plays a more important part in their control than it does in the control of theoretical laws. On the other hand, theoretical laws are 'more theoretical' in the sense that they come closer to the theoretical ideals of inquiry.

A settlement of the issue between conventionalists and empiricists can be reached by recognizing that there are two types of 'law' and by determining their relations. All the frequently advanced arguments in which conventionalism is contrasted, as subjectivism or idealism, with empiricism, as objectivism or realism, miss the essential methodological point.

Some remarks concerning the extent to which the ideals of inquiry are fulfilled in contemporary physics may be appropriate here. As we have already mentioned, the ideals combined in the conception of a rational cosmos seemed to be almost attained in the system of pre-quantum physics. Classical physics, in the form it has received in Einstein's theory of relativity, comes indeed close to these ideals. However, it should be noted that theoretical laws rather than empirical laws satisfy these standards. The visitor who enters the magnificent building of physics through the front door, where the 'theorist' exhibits his work, finds things more perfectly arranged than the visitor who comes through the back door, which leads to the laboratory.

But the results of the experiments leading to the development of quantum physics did not fit into the frame of classical physics.

Planck's and Einstein's explanation of the phenomena of radiation, which could not be interpreted in terms of Maxwell's theory, led to revolutionary changes in the foundations of physics. According to the theory now prevailing,<sup>10</sup> physical laws can be divided into two classes:

1. *The laws of classical physics in the form they have received in the theory of relativity.* They prove to be adequate for the treatment of phenomena of macroscopic magnitude. These 'molar' objects are thought of as composed of a large number of microscopic constituents, the behavior of which, however, cannot be described adequately by 'molar laws.' Molar laws fulfil to a high degree the ideals of unity and simplicity. The laws exhibit a clear hierarchical organization; the mathematical functions are continuous, and their predominant form is that of differential equations of the second order.

2. *Quantum laws.* They must be introduced in order to describe the behavior of microscopic particles, particularly electrons and protons, the elementary electrical units. It is presumed that the quantum laws form a unity among themselves. On the other hand, their structure differs greatly from that of the laws of classical physics. The principle of continuity collapses, and furthermore the mathematical functions of quantum physics are far less 'simple' than those of classical physics. Niels Bohr's 'correspondence principle' represents an attempt to clarify the relation between quantum laws and the laws of classical physics. According to it the microscopic laws converge toward molar laws when the number of particles or quanta is very large. Molar laws are thus interpreted as statistical laws relating to very large aggregates of microscopic particles or quanta.

There is still disagreement among the foremost living physicists about whether we may hope to restore the unity of physics.

Mathematical form is generally regarded as essential for the laws of physics. The desire to prove the 'rationality' of the laws of physics favored the erroneous view that the truth of the laws of physics is vouchsafed by virtue of their having a 'simple' mathematical form. We have emphasized in Chapter III that

this error is due to the failure to distinguish properly between pure mathematics and applied mathematics, i.e. between analytic propositions of a particular kind and synthetic propositions. This point is so important for the understanding of science that we must dwell upon it a little longer.<sup>11</sup>

It will be well to summarize briefly some of the chief results of the painstaking analyses concerning the foundations of mathematics that were performed by logicians and mathematicians during the last century.

(a) There is no fundamental distinction in pure mathematics between pure arithmetic and pure geometry. The latter is reducible to the former.

(b) All the concepts of pure mathematics are definable in terms of the series of integers (1, 2, 3 . . .), and the whole theory of integers can be derived from the following postulates:

1. There exists a first element.
2. Every element has just one immediate successor.
3. Every element except the first has just one immediate predecessor.

4. The series of integers is *completely* defined by postulates 1-3. (Equivalent of the principle of perfect induction.)

(c) There are no 'infinitely small magnitudes' in mathematics. All terms of the infinitesimal calculus can be 'translated' into finite language.

(d) The notion of the infinitely great, the actual infinite of Cantorian set theory, has not yet been completely abandoned, but I have little doubt that here too the finitist view will eventually prevail.

Until a few decades ago it was almost generally held that irrefutable knowledge of the geometrical properties of physical space is intuitively given and confirmable by any sort of direct inspection of physical objects, and that, accordingly, Euclidean geometry is a system of absolutely certain propositions about reality.

But this is an erroneous view. It is now definitely established that pure geometry can be interpreted as a system of *proposi-*

*tional functions*<sup>12</sup> by which logical and arithmetical relations among otherwise undetermined classes of objects (e.g. 'points,' 'lines,' 'planes') are established. No reference to facts is here involved. Only when the question is raised whether a particular geometry is suited for the description of physical reality is such reference made. For Euclidean geometry this question was answered in the negative by Einstein.

It is found that one of the axioms, the so-called Euclidean parallel postulate, has to be replaced by another. In this sense, the geometry of modern physics, as it is presented in Einstein's general theory of relativity—the Riemannian geometry—is non-Euclidean. For a given 'straight line' there is no other 'straight line' in a 'plane' with which it does not have a 'point' in common. But Euclidean geometry remains applicable in weak gravitational fields like that of the earth.<sup>13</sup>

When we realize that the mathematical form of physical laws by no means confers 'necessary validity' upon them, we shall be less inclined to draw a sharp line of demarcation between laws that have and laws that do not have mathematical form. It has frequently been said that only the former are 'true' laws, which means that the term 'law' should be reserved exclusively for them. But this restriction on its use would tend to make us disregard the essentially similar role of both types of law in empirical procedure.

Once the meaning of 'law' is clarified, substantial difficulties are no longer encountered in the attempt to understand 'causality.'<sup>14</sup> The important thing is to realize that causal relations are defined in terms of laws. Let us first consider the case in which the law to be referred to is an empirical law, a synthetic universal proposition. Now let  $L$  be a law of the form: 'Wherever and whenever events of the kind  $e_1$  occur, they will be succeeded by events of the kind  $e_2$  at a certain (more or less precisely determined) spatio-temporal distance.' Then we shall call events of the kind  $e_1$  'causes of events of the kind  $e_2$  in terms of  $L$ .'

For the sake of simplicity we have in the preceding definition referred to  $e_1$  and  $e_2$  as single events, but they need not be

considered as simple facts. Now let  $e_1$  be a compound of a finite number of facts,  $f_1, f_2, \dots f_n$ . We shall then call this set of facts a *basic cause* of  $e_2$  if none of its subsets can replace it in the causal law under consideration without affecting its validity. This law will then be called a *basic causal law*. A basic causal law for  $e_2$  implies logically an unlimited number of non-basic causal laws, namely, all those in which other events are added to the set of events that constitute a basic cause. All these sets of facts are *sufficient conditions* for the occurrence of an event of the kind  $e_2$  at a determined spatio-temporal distance from them. But only the basic cause can be called a *necessary condition* for the occurrence of  $e_2$ , because each of the facts constituting the basic cause is indispensable. But this use of the term 'necessary condition' is not advisable, since it may lead to the erroneous belief that there can be only one basic causal law in terms of which the occurrence of an event of the kind  $e_2$  at a particular place and time may be explained.

Another meaning of the term 'necessary condition' must be clearly distinguished from the one just mentioned. It is related to causal laws of the form: Events of the kind  $e_2$  will always occur if, and never occur unless, events of the kind  $e_1$  have occurred before. Only causes of  $e_2$  that are defined in terms of basic causal laws of this kind should be called *necessary and sufficient* conditions for the occurrence of  $e_2$ . It is an ideal of inquiry to explain events exclusively in terms of such laws. If the possibility of attaining the ideal is taken to be vouchsafed by the 'nature of things,' one is prone to declare that only such conditions of an event are its 'true' causes as are necessary and sufficient conditions. But this restriction would not be in accordance with the generally accepted use of the term 'cause.' Moreover, it might lead to a revival of the erroneous view that there can be only one true cause of a given event, whereas in fact an indefinite number of laws establishing necessary and sufficient conditions for its occurrence might be found.

A weaker restriction of the meaning of 'cause' would be its identification with 'basic cause.' But since the term is often used



in the broader sense covered by our definition, such a restriction does not seem to be appropriate either. But this is a terminological issue of minor significance. The essential point in determining the meaning of 'cause' is to realize that it is elliptical to speak of a cause of a given event without referring explicitly to the law in terms of which it is a cause of the event.

There is finally a connotation of the term 'necessary' as related to 'cause' that represents another instance of the confusion of deductive reasoning with empirical procedure. According to it, the effect will by necessity come into existence when the cause is actualized, since it is 'contained in' the cause. Now we have seen that it is incompatible with the principle of permanent control to assign necessary validity to any statement of fact. Statements of causal relation can therefore not be taken to mean that the occurrence of a fact is necessarily established by the previous occurrence of another fact.

But we can interpret 'necessary relation of cause and effect' in a different and methodologically significant way in terms of our distinction between empirical laws and theoretical laws. To do so, we have first to replace the sentence 'If the cause  $e_1$  of  $e_2$  occurs,  $e_2$  will necessarily follow' by the sentence 'If in one instance after the occurrence of  $e_1$ ,  $e_2$  does not occur (at the determined spatio-temporal distance from  $e_1$ ), we can no longer call  $e_1$  a cause of  $e_2$ .' If we consider further the relativity of cause to law, we realize that this is tantamount to saying that the underlying causal law is falsified by one negative instance—in other words, that it is an empirical law. From an empirical law combined with a pertinent statement of fact a prediction can be deduced. It is therefore correct to say that such a prediction is logically contained in (necessarily implied by) these two propositions, which are presupposed in the meaning of 'cause.' But the fulfilment of such a prediction is not ascertained thereby.

If, on the other hand, 'cause' is defined in terms of a theoretical law, then we have no such necessary relation between 'cause' and 'effect.' Since a theoretical law may be upheld when a prediction in terms of it is not fulfilled, events of a kind  $e_1$  may still

be called 'causes,' in terms of a theoretical law, of events of a kind  $e_2$  if it is found that an  $e_1$  was not succeeded by an  $e_2$ .

We have thus to distinguish between causal explanation in terms of empirical laws and causal explanation in terms of theoretical laws. The former is a deduction from accepted propositions. In contrast to the latter, it does not involve any step.

With Hume's critical analysis of causality in mind, a number of modern philosophers and scientists have suggested that we abandon the use of this term and replace it by 'correlation' or 'function.'<sup>15</sup> While this change in terminology may aid us in avoiding some traditional errors, it may lead, and indeed has led, to disregard of the highly complex structure of the rules of procedure governing the acceptance and elimination of the causal laws. It is apt to suggest statistical investigations without a well-established theoretical basis and over-emphasis on induction by simple enumeration. This crude form of empiricism constitutes a veritable danger to the social sciences, where the theoretical foundations are not so firmly established as in natural science.

Some brief remarks concerning the meaning of the 'principle of causality' are still to be made. In the form, 'Every event has a cause,' it is an expression of the methodological resolution not to regard any fact as unexplainable in principle and of the belief that the attempt to find an explanation is not doomed to failure.<sup>16</sup> If the principle of causality is meant to contain additional requirements, e.g. that of continuity, then it expresses the methodological resolution to accept only explanations satisfying these requirements, or at least to give preference to such explanations. Recent developments in physics have indicated that such postulates are no longer regarded as immutable.

The conflict in which determinists and indeterminists have opposed each other for more than two millennia centers about the question whether the principle of causality also holds for the psycho-physical world or whether it is abrogated there on account of freedom of the will. The methodological implications of this question will be discussed in Chapter XIII.

WE have emphasized in the preceding chapter that the idea of immediate apprehension of truth by acts of perception cannot bear close examination. However, it is still defended by prominent philosophers. One of the foremost contemporary proponents of this doctrine is Bertrand Russell.

In his latest philosophical work, *An Inquiry into Meaning and Truth*,<sup>1</sup> he writes:

In recent philosophy we may distinguish four main types of theory as to 'truth' or as to its replacement by some concept which is thought preferable. These four theories are:

I. The theory which substitutes 'warranted assertability' for 'truth.' This theory is advocated by Dr. Dewey and his school.

II. The theory which substitutes 'probability' for 'truth.' This theory is advocated by Professor Reichenbach.

III. The theory which defines 'truth' as 'coherence.' This theory is advocated by Hegelians and certain logical positivists.

IV. The correspondence theory of truth, according to which the truth of basic propositions depends upon their relation to some occurrence, and the truth of other propositions depends upon their syntactical relations to basic propositions.<sup>2</sup>

Russell professes his firm adherence to the correspondence theory and undertakes to support it by an analysis of basic propositions. He characterizes a basic proposition by the following two properties:

- (1) It must be caused by some sensible occurrence;
- (2) It must be of such a form that no other basic proposition can contradict it.<sup>3</sup>

He remarks to (1), 'I do not wish to insist upon the word "caused" but the belief must arise on the occasion of some sensible occurrence, and must be such, that, if questioned, it will be defended by the argument "Why, I see it" or something similar.'<sup>4</sup>

It appears from this remark that Russell does not mean to state that there is a causal relation between a sensible occurrence and a *proposition* (which is inconceivable), but rather that there is such a relation between the former and a *belief in the validity of the proposition*. He then holds that the genesis of this belief is also the criterion of its correctness. Thereby he tacitly assumes that the existence of the causal relation between a sensible occurrence and a belief is immediately given, which is an untenable view. Furthermore, an intrinsic similarity between the nature of the sensible occurrence and the meaning of the proposition believed is obviously presupposed. Various beliefs may arise 'on the occasion of some sensible occurrence,' but Russell surely does not intend to claim that they are all indubitably evident. This he claims only for the belief in the proposition that 'corresponds' to the sensible occurrence. But what is meant by 'correspondence'? According to sensationalist doctrine, of which Russell's is perhaps the most subtle contemporary variety, 'correspondence' should be interpreted in the sense that the proposition states (or reproduces) the content of the sensation. Such an interpretation implies that sensations have objective content; otherwise they could not be reproduced by objective conceptual or propositional meanings.

An example of a basic proposition given by Russell is: 'That is red.'<sup>5</sup> It contains the objective meaning 'red,' which is presupposed as identical in all red-perceptions had by a potentially infinite number of persons. But Russell states, 'All basic propositions in the above sense are personal, since no one else can share my percepts, and transitory, for after a moment they are replaced

by memories.'<sup>6</sup> Nevertheless, they are taken to represent the indubitable basis on which the edifice of science rests. That this view is advanced by one of the greatest living logicians shows how deep-rooted sensationalism still is in some of the best minds of our time. It is not clearly enough recognized that results of inference (in the broadest sense) are implicit in all statements about facts, and that the observational test is interrelated with other controls.

Our argument may be summarized as follows: If a correspondence theory proposes as a criterion of the truth of a synthetic proposition its agreement with things as they are in themselves, then it offers a criterion devoid of procedural significance. If, however, agreement between synthetic propositions and percepts is taken to be the criterion, then it is not clear how truth can be regarded as objective.

The coherence theory of truth in the form now given to it by the logical positivists is not essentially different from Dewey's theory, which replaces 'truth' by 'warranted assertability,' though there are significant differences between the two doctrines in other respects. If the logical positivists tended at any time to interpret 'coherence' as 'logical consistency,' they do so no longer. Explication of the meaning of 'coherence' or warranted assertability leads to the formulation of the rules of empirical procedure discussed in previous chapters. This has been almost generally disregarded because of the still prevailing erroneous view that truth and falsity are somehow 'contained in' the propositional meanings. While this view is much closer to the correspondence than to the coherence theory of truth, it has not been quite eliminated from the latter and has impeded its elaboration. 'Coherence' must be defined in terms of rules of procedure and understood either as 'empirical validity' or as 'the ideal of truth.'

This issue is often raised by asking whether truth depends on knowledge. Proponents of the correspondence theory are prone to put the question in this form, thereby suggesting that the coherence theory stands or falls with the thesis that knowledge 'makes' truth, which is then declared inconsistent with common

sense as well as with the results of philosophical reflection. If 'fact' is substituted here for 'truth,' we have a typical argument of realists against idealists, though many advocates of a coherence theory would profess themselves to be realists rather than idealists.

It should be clear from our analysis in the preceding chapters that the question is misleading as it stands. Its formulation makes it seem as if the problem were whether a causal relation between truth (or fact) and knowledge goes from truth (fact) to knowledge or from knowledge to truth.

But the really significant methodological issues are:

1. Should 'truth' be defined in terms of 'knowledge' (or experience) or vice versa, and,
2. if the first alternative is chosen (as I think it must be), should 'truth' be defined in terms of actual knowledge or of possible knowledge?

As to question (2) we have made it clear in Chapter iv that there are two conflicting uses of the term 'truth' related to synthetic propositions, namely, accomplished verification and verifiability.<sup>7</sup>

It is the confusion between logical relations and causal relations that makes the coherence theory of truth seem inadequate; not only the issue of the 'priority' of knowledge over truth but many other 'priority' controversies have been obscured by failure to distinguish clearly between these two kinds of relations.

The correspondence theory of truth is closely associated with the view that feelings of evidence are criteria of truth. This view is untenable. A controversy between scientists is obviously not decided by comparing the strength of the beliefs of the parties involved. By keeping this fact in mind in the analysis of 'probability,' to which we now turn, we shall be able to avoid some of the errors to be found in the traditional treatment of the problem.

If 'truth' is interpreted as perfect (absolute) certainty in a psychological sense, it seems reasonable to interpret probability as imperfect certainty and to relate different degrees of probability to different intensities of belief. Such interpretations are

also suggested by the view that probability propositions are a makeshift required by reason of human finitude. Where we are incapable of discovering strict laws, which completely determine every single event, we must resign ourselves to more or less rough conjectures. In these instances we do not have genuine knowledge but only a more or less intense belief. Different numerical values of probability are interpreted as measures of this intensity. But such an interpretation does not contribute in the least to the logical analysis of probability propositions. Neither their meaning nor the rules applying to their control nor their function in the control of other propositions is thereby clarified. Yet it is these issues alone that are relevant for the logic of science. There is, however, a methodologically significant connotation of the term 'absolute certainty,' namely, the irreversibility of results arrived at by a procedure. A *correctly* demonstrated theorem in pure mathematics is proved once for all, but a proposition correctly incorporated into an empirical science is not exempt from future elimination.<sup>8</sup>

The principal objection to an interpretation of probability in terms of the contrast between imperfect and perfect knowledge is that it does not take proper account of the fundamental difference between analytic and synthetic propositions. It is inappropriate to view analytic propositions—which contain no assertion about reality—as perfect knowledge simply because they are exempt from refutation by a control that by presupposition cannot be applied to them. As we have already emphasized in Chapter IV, we point to a difference in kind, not in degree of perfection, in contrasting the validity of rational knowledge with that of empirical knowledge.

The fundamentals of the sensationalistic doctrine are accepted in one of the outstanding modern works on probability, J. M. Keynes' *Treatise on Probability*.<sup>9</sup> His basic philosophical view is very close to Russell's. In what follows we shall try to show that Keynes' theory of probability inference proves to be in essential agreement with some of the results of our analysis of scientific procedure when the conclusions that he draws from his basic

sensationalistic tenets are dropped. Since Keynes expresses his ideas with admirable clarity, it will be appropriate to present them in his own words:

Part of our knowledge we obtain direct; and part by argument. The Theory of probability is concerned with that part which we obtain by argument, and it treats of the different degrees in which the results so obtained are conclusive or inconclusive . . .<sup>10</sup>

Given the body of direct knowledge which constitutes our ultimate premisses, this theory tells us what further rational beliefs, certain or probable, can be derived by valid argument from our direct knowledge . . .<sup>11</sup> The terms *certain* and *probable* describe the various degrees of rational belief about a proposition which different amounts of knowledge authorise us to entertain. All propositions are true or false, but the knowledge we have of them depends on our circumstances; and while it is often convenient to speak of propositions as certain or probable, this expresses strictly a relationship in which they stand to a *corpus* of knowledge, actual or hypothetical, and not a characteristic of the propositions in themselves. A proposition is capable at the same time of varying degrees of this relationship, depending upon the knowledge to which it is related, so that it is without significance to call a proposition probable unless we specify the knowledge to which we are relating it . . .<sup>12</sup>

Let our premisses consist of any set of propositions *h*, and our conclusion consist of any set of propositions *a*, then, if the knowledge of *h* justifies a rational belief in *a* of a degree  $\alpha$ , we say that there is a *probability-relation* of degree  $\alpha$  between *a* and *h* . . .<sup>13</sup>

Thus, when in ordinary speech we name some opinion as probable without further qualification, the phrase is generally elliptical . . .<sup>14</sup>

If a man believes something for a reason which is preposterous or for no reason at all, and what he believes turns out to be true for some reason not known to him, he cannot be said to believe it *rationally*, although he believes it and it is in fact true. On the other hand, a man may rationally believe a proposition to be *probable*, when it is in fact false. The distinction between rational belief and mere belief, therefore, is not the same as the



distinction between true beliefs and false beliefs. The highest degree of rational belief, which is termed *certain* rational belief, corresponds to *knowledge*. We may be said to know a thing when we have a certain rational belief in it, and *vice versa* . . .<sup>15</sup>

We start from things, of various classes, with which we have, what I choose to call without reference to other uses of this term, *direct acquaintance*. Acquaintance with such things does not in itself constitute knowledge, although knowledge arises out of acquaintance with them. The most important classes of things with which we have direct acquaintance are our own sensations, which we may be said to *experience*, the ideas or meanings, about which we have thoughts and which we may be said to *understand*, and facts or characteristics or relations of sense-data or meanings, which we may be said to *perceive*;—experience, understanding, and perception being three forms of direct acquaintance . . .

The objects of knowledge and belief—as opposed to the objects of direct acquaintance which I term sensations, meanings and perceptions—I shall term *propositions* . . .

Now our knowledge of propositions seems to be obtained in two ways: directly, as the result of contemplating the objects of acquaintance; and indirectly, *by argument*, through perceiving the probability-relation of the proposition, about which we seek knowledge, to other propositions. In the second case, at any rate at first, what we know is not the proposition itself but a secondary proposition involving it. When we know a secondary proposition involving the proposition *p* as subject we may be said to have indirect knowledge *about p*.

Indirect knowledge about *p* may in suitable conditions lead to rational belief in *p* of an appropriate degree. If this degree is that of certainty, then we have not merely indirect knowledge *about p*, but indirect knowledge *of p* . . .<sup>16</sup>

It is not always possible, however, to analyse the mental process in the case of indirect knowledge, or to say by the perception of *what* logical relation we have passed from the knowledge of one proposition to knowledge about another. But although in some cases we *seem* to pass directly from one proposition to another, I am inclined to believe that in all legitimate transitions of this kind some logical relation of the proper kind must exist

between the propositions, even if we are not explicitly aware of it.<sup>17</sup>

Now it can easily be seen in the light of our preceding analysis that the only certain knowledge really presupposed in probability inference is the understanding of meanings. No supposition of a direct experience that yields absolutely certain premises about matters of fact must be made. Such a supposition would imply that some classes of synthetic propositions are exempt from permanent control, which is not in keeping with the fundamentals of empirical procedure. However, Keynes' definition of 'probability' quoted above is independent of the view that absolutely certain knowledge can be attained by direct acquaintance. If in Keynes' analysis we replace 'evidence' by 'grounds,' 'rational belief' by 'correct scientific decision,' and 'secondary propositions' by 'rules of procedure,' the parallelism between Keynes' approach and our own analysis in Chapter IV becomes apparent. The puzzling problem of the foundations of the 'certainty' of secondary propositions<sup>18</sup> disappears as soon as we realize that they are implicitly presupposed rules of procedure. But Keynes' thesis, that in all legitimate transitions performed in the acquisition of indirect knowledge logical relations must hold between the propositions concerned, cannot be accepted as it stands. What is required is rules of procedure in terms of which we can distinguish between legitimate and illegitimate transitions, or, in our terminology, between correct and incorrect scientific decisions. The relations established by these rules are, as we have seen, fundamentally different from the internal relations among propositions in deductive logic.

This trend of Keynes' argument, which, by contrasting deductive inference and probability inference, points toward a logical analysis of empirical procedure in general, interfuses with another trend, which is indicated by his notion of degree of probability. 'Degree of probability' is related to 'verification'; different degrees of probability are different degrees of confirmation in given scientific situations. In examining the procedural signifi-

cance of this notion, we have first of all to recognize that a proposition cannot be verified to a certain degree; but the conditions for its verification may be more or less complex and may be entirely or only partly fulfilled in a given scientific situation. Thus, we may distinguish between 'complete confirmation' (i.e. verification) and 'incomplete confirmation.'<sup>19</sup>

We shall call a *complete verifier* of a proposition  $p$  any set of propositions that is a sufficient, but not a redundant, basis for the acceptance of  $p$ . A proposition may have different complete verifiers. Any proper subset of a complete verifier of  $p$  may be called an *incomplete verifier* of  $p$ . Considering that a larger subset of a complete verifier of  $p$  confirms  $p$  to a higher degree than a smaller subset of it, we arrive at the following definition of 'degree of confirmation' ('degree of probability'):

Let  $v_c$  be a complete verifier of a proposition  $p$  undecided in each of the two scientific situations  $s_1$  and  $s_2$ . Let  $v_1$  and  $v_2$  be the two largest subsets of  $v_c$  contained in  $s_1$  and  $s_2$  respectively. We then say that  $p$  is confirmed to a higher degree by  $s_1$  than by  $s_2$  *in terms of*  $v_c$  (that  $s_1$  gives  $p$  a higher degree of probability than  $s_2$  *in terms of*  $v_c$ ), if and only if  $v_2$  is a proper subset of  $v_1$ ; in other words, if, and only if,  $v_1$  logically implies  $v_2$ , but not conversely.

If  $p$  is confirmed by  $s_1$  to a higher degree than by  $s_2$  in terms of at least one complete verifier, and if it is *equally* confirmed by  $s_1$  and  $s_2$  in terms of all other complete verifiers (which means that the corresponding incomplete verifiers are identical), then we may say that  $p$  is confirmed by  $s_1$  to a higher degree than by  $s_2$ , without explicitly referring to complete verifiers. But we must constantly keep in mind that such a formulation is elliptical.

*It is thus seen that reference to the criteria of verification, i.e. to the basic rules of scientific procedure is required in the definition of 'degree of probability.'*

Since the term 'probability' as used in this context refers exclusively to undecided propositions, it has a meaning different from the one ascribed to it when we say that all synthetic propositions are 'merely probable,' or that all synthetic propositions

knowledge of which is not attainable by direct acquaintance are 'merely probable.' This use of the term 'probable,' as we have already mentioned, means that accepted propositions are never exempt from *invalidation*, whereas 'degree of probability' refers to the criteria of *verification*.

Another point to be stressed is that the definition above of 'degree of confirmation' ('degree of probability') refers to single propositions and the evidence for them in given scientific situations. However, there are problems of comparing the 'degree of probability' of different propositions in the same scientific situation. We may pose the problem: Given two undecided propositions and the evidence for each of them, to determine which one is 'preferable' to the other. The most significant instances are those in which the two propositions are incompatible exclusive of each other. The practical man and the scientist are often confronted with decisions of this kind, and they do in fact critically discriminate between correct and incorrect decisions in such cases. In determining the direction of his research, the scientist will often have to ask himself: Is it correct to adopt an undecided proposition  $p_m$  rather than an undecided proposition  $p_n$  incompatible with  $p_m$ , as a working hypothesis?

We may define the term 'more probable' as related to two propositions as follows: To say with respect to two propositions  $p_m$  and  $p_n$  undecided in a given scientific situation  $s$  that  $p_m$  is *more probable* than  $p_n$  (has a *higher degree of probability* than  $p_n$ ) is to state that  $p_m$  is preferable to  $p_n$  as a working hypothesis. To say simply that  $p_m$  is probable means that it is preferable to its negation.

The problem is then to clarify the implicitly presupposed criteria for decisions of this kind. The pertinent rules may be called rules of *probability preference*. It will not do to dispose of this problem by maintaining that the rules are not 'given.' If a scientist declares that it is justified to adopt one undecided proposition rather than another as a working hypothesis, he is bound to give objective reasons for his statement and these

imply, as we have seen, reference to presupposed rules of procedure.

The relation between degree of confirmation and probability preference is easily understood. Let  $p_1$  be a proposition correctly preferred to a proposition  $p_2$  in a scientific situation  $s_1$ . Then  $p_1$  will also be preferable to  $p_2$  if in a new scientific situation  $s_2$  additional evidence is offered for  $p_1$  whereas that for  $p_2$  remains unchanged; but if, by elimination of a previously accepted proposition,  $p_1$  is confirmed to a lower degree in the situation  $s_2$ , then the preference may have to be reversed.

We have so far distinguished between two different meanings of the term 'probability,' one relating to empirical knowledge as such—when it is contrasted with rational knowledge—the other to synthetic propositions undecided in a given scientific situation. It is common to both these meanings that they do not refer to any particular content of the propositions under consideration.

The opposite holds for the *frequency interpretation* of 'probability,' to which we now turn. There is hardly any doubt that this interpretation is adequate as far as the term 'probability' relates to statistical laws. However, one of the chief problems of probability logic is in what way (if any) the frequency interpretation is related to the second of the interpretations just mentioned.

Keynes holds that his conception of probability is wider than the frequency conception. Reichenbach states that the frequency interpretation applies in all cases in which we speak of the probability of single events or propositions. Carnap, Popper, and a number of other logicians maintain that the frequency conception of probability is unrelated to those meanings of the term 'probability' that refer to single facts or propositions. They assert however, that both types of meanings have procedural significance. This is denied by R. von Mises, one of the leading contemporary proponents of the frequency interpretation. He declares that we must free ourselves from the idea connected with everyday language that probabilities may be assigned to single

events. We shall briefly summarize his widely accepted interpretation of the frequency theory.

The meaning of 'probability'—von Mises declares—presupposes that of '*collective*,' i.e. 'a mass phenomenon or a repetitive event, or, briefly, a long series of observations for which there are sufficient reasons to believe the hypothesis that the relative frequency of an attribute would tend to a fixed limit if it were indefinitely continued. This limit will be called *The probability of the attribute considered within the given collective*.'<sup>20</sup> He offers as an example of a collective: 'All men insured before reaching the age of forty after complete medical examination and with the normal premium.'<sup>21</sup>

A collective appropriate for the application of the theory of probability must fulfil two conditions. First, the relative frequencies of the attributes must possess limiting values. Second, these limiting values must remain the same in all partial sequences which may be selected from the original one. Of course, only such sequences can be taken into consideration which can be extended indefinitely, in the same way as the original sequence itself.<sup>22</sup>

An example of this kind is the partial sequence formed by all odd numbers of the original sequence.<sup>23</sup>

The only essential condition is that the question as to whether a certain member of the original sequence belongs to the selected partial sequence or not should be settled independently of the result of the corresponding observation, i.e. before anything is known about this result. We shall call a selection of this kind a *place selection*. The limiting values of the relative frequencies in a collective must be independent of all possible place selections.<sup>24</sup>

'The purpose of the theory of probability is exclusively the calculation of probability distributions in collectives derived by means of given distributions in the initial collectives.'<sup>25</sup> This calculation is nothing else than a mathematical transformation of the given distribution. Pure probability mathematics (a branch of combinatorics) is to applied probability mathematics

what pure geometry is to applied (physical) geometry. Theorems of pure geometry (e.g. the Pythagorean theorem) do not make any assertion about reality, and the same holds for theorems of pure probability mathematics, e.g. Poisson's theorem.<sup>26</sup> Statistical theory is applied probability mathematics.

Von Mises' frequency interpretation of probability appears to be in accord with statistical theory. But the application of the calculus of probability to empirical science requires further clarification. The concept of limit is one of pure mathematics and is defined exclusively for *infinite* series. A collective, however, consists of a *finite* series of events, so that the concept of limit seems to be inapplicable to it. As a consequence no 'probability law' is falsifiable. Nagel offers the following example:

Suppose we test the hypothesis that the probability of heads is  $\frac{1}{2}$  by flipping the coin a thousand times, and suppose we get a run of a thousand heads. We might be inclined to conclude that the hypothesis has been definitely proved erroneous. However, on that very hypothesis such a run of heads is not excluded, since that hypothesis asserts something about the limiting ratio of heads in an *infinite* class and not in a *finite* one. In general, that hypothesis is compatible with *any* results obtained in any finite number of throws; and conversely, a given result within a given class of throws is compatible with any hypothesis about the numerical values of the probability. In short, it seems that no statistical evidence obtainable from actual trials (which must obviously be finite in number) can establish or refute a probability statement.<sup>27</sup>

This is tantamount to saying that the frame of possibilities is not restricted by a probability law.

This result seems strange at first sight. Successful predictions in terms of probability laws are not only made by insurance companies and gambling institutions, which were among the first to apply probability theory for their purposes, but they play a predominant part in modern physics, and their significance for biology, psychology, and the social sciences is steadily increasing.

But the paradox disappears if we apply the distinction between

theoretical laws and empirical laws explained in the last chapter, and between procedural rules of different orders. We then see that a probability law is not meant to be a statement about reality, i.e. a synthetic proposition, but a *scheme for the formation of specific rules of procedure*, i.e. a procedural rule of higher order. The specific rules determine the meaning of warranted predictions concerning relative frequencies in finite series of events forming collectives in von Mises' sense.

In speaking of probability laws as schemes for the formation of specific rules of procedure, we mean that these rules must satisfy certain conditions stated by the law. The relative frequency referred to in the specific rule must not deviate too far from the limit of relative frequencies referred to in the law, and as the number of instances is increased, it must approach closer and closer to the limit. To state more precisely what 'too far' and 'closer and closer' mean in this context would require analysis of the body of theoretical principles implicitly presupposed in the transition from a probability law to such a specific rule. This cannot be attempted here.

Probability laws may have to be eliminated if a considerable number of predictions in terms of the related specific rules do not withstand observational tests or if they no longer fit into the theoretical frame. But they cannot be falsified. As theoretical laws of higher order, probability laws cannot be directly applied to predictions. They only 'impose conditions' upon the formation of theoretical laws that can be so applied.

Statements concerning the relative frequency of events belonging to a certain subclass of a given class of events do not refer to modes of validity or degrees of rational belief. Von Mises is therefore right in insisting upon a strict separation between statistical theory and modality logic and in rejecting the direct application of the frequency concept of probability to single propositions or facts. On the other hand, it must not be overlooked that we can and actually do correlate procedural rules of probability preference concerning single propositions with statistical laws. We should say, for instance, that it is correct



to prefer the statement that a number other than 1 will appear on the face of a die at the next throw to the statement that 1 will appear.

It is to the credit of Reichenbach to have duly stressed this point. In discussing it, he introduces the term 'weight': '*A weight is what a degree of probability becomes if it is applied to a single case.*'<sup>28</sup> In analyzing this definition, we have to note that the numbers thus assigned to single propositions must not be regarded as cardinal numbers capable of addition, multiplication, etc. We can make this clear by considering other instances of correlation between intensive and extensive magnitudes. Different numbers of vibrations (per unit of time) of a string correspond to different pitches of the sound produced by the string; for instance, to a sound  $s_2$  one octave higher than a sound  $s_1$  there corresponds twice the number of vibrations. We should not say, however, that  $s_2$  is twice as high as  $s_1$ . Nor would it be appropriate to say that a consumer likes a unit of a commodity  $c_2$  twice as much as a unit of a commodity  $c_1$  if he is prepared to pay twice as much for it. Nevertheless, in each of these cases the place of any given object in a scale of intensive magnitudes is completely determined by the number assigned to it. Thus, we may say that we can establish systems of *probability indices* for single propositions or single events in terms of statistical laws. Use of the term 'probability index' instead of 'weight' may be advisable in order to avoid the connotation of quantity usually connected with the term 'weight.'

But it is important to note that the *probability indices are intrinsically related to rules of probability preference* and therefore cannot be *deduced* from the statistical laws, even though their numerical value is determined by those laws. In other words, 'probability index' is not defined in terms of statistical laws alone. Specific rules of probability preference are presupposed.

It is thus seen that rules of probability preference correspond to every statistical law. But what about the converse relation? Is there a statistical law 'behind' every probability preference?

Reichenbach, who calls his own view the 'identity conception of probability' as contrasted with the 'disparity conception' of other doctrines, answers in the affirmative. He declares that 'ordinary language suppresses [in probability statements about single cases] a reference to a class, and speaks incorrectly of a single event where a class of events should be considered,'<sup>29</sup> and argues:

Why do we ascribe, say, a high probability to the statement that Napoleon had an attack of illness during the battle of Leipzig, and a smaller probability to the statement that Caspar Hauser was the son of a prince? It is because chronicles of different types report these statements: one type is reliable because its statements, in frequent attempts at control, were confirmed; the other is not reliable because attempts at control frequently led to the refutation of the statement. The transition to the type of the chronicle indicates the class of frequency interpretation, the probability occurring in the statements about Napoleon's disease, or Caspar Hauser's descent, is to be interpreted as concerning a certain class of historical reports and finds its statistical interpretation in the frequency of confirmations encountered within this class . . .<sup>30</sup>

It has been argued that in such cases we know only a comparison of probabilities, a 'more probable' and 'less probable' . . .

This is not false; it is certainly easier to know determinations of a topological order than of a metrical character. The former, however, do not exclude the latter; there is no reason to assume that a metrical determination is impossible. On the contrary, the statistical method shows ways for finding such metrical determinations; it is only a technical matter whether or not we can carry it through.

There are a great many germs of a metrical determination of weights contained in the habits of business and daily life. The habit of betting on almost everything unknown but interesting to us shows that the man of practical life knows more about weights than many philosophers will admit.<sup>31</sup>

Let us now examine this view. Reichenbach is right (as is Keynes, one of the foremost proponents of the disparity view)

in stressing the ellipticity of the usual formulations of probability statements concerning single cases. But the crucial question is whether the removal of the ellipticity will in all cases reveal an underlying system of metrical relations, and this question, I think—in agreement with Keynes—must be answered in the negative. To say this is not to endorse a radical variety of the disparity view according to which it would be *a priori* impossible to establish such metrical relations. It is not even to deny that the task of establishing metrical relations is *indicated* wherever we have probability statements concerning single propositions or single cases. What we deny is only that metrical relations are logically implied in all probability statements of this kind, as they would have to be according to the identity conception. Reichenbach's thesis that it is only a technical matter whether or not we can find metrical relations is not free from ambiguity. If it is interpreted as merely a refutation of the thesis that such an attempt is in certain cases *a priori* doomed to failure, then it must be accepted. If, however, his thesis is taken to mean that metrical relations are always implicitly presupposed in probability statements, then it must be rejected.

Reichenbach's view on this point is related to his ingenious conception of an infinite-valued probability logic.<sup>32</sup> The probability concept—he argues—is a generalization of the truth-concept. According to traditional logic every proposition has one and only one of the two truth-values 'true' and 'false.' They may be symbolized by the numbers 1 and 0, and classical logic may be presented in the form of a calculus by which operations with these two numbers are defined.

In a similar way one may construct calculi of three-valued logic, for which the trichotomy true-false-indeterminate is a model, and generally calculi of *n*-valued logic, where *n* need not even be finite. In Reichenbach's probability logic all real numbers between 0 and 1 are possible values assignable to propositions as appraised weights. We cannot, in this context, examine his highly elaborated theory in all its details. We must confine ourselves to one important point that falls within the scope of

our analysis. Reichenbach links the notion of probability logic with the thesis that all propositions in empirical science are 'merely probable.'

Before the throw of the die, we have only a probability statement about the result of the throw; after the throw we say that we know the result exactly. But, strictly speaking, this is only the transition from a low to a high probability; it is not absolutely certain that there is a die before me on the table showing the side 1. The same is valid for any other proposition whatever . . .<sup>38</sup>

The methodological core of this statement is that no proposition accepted in science is exempt from invalidation or even falsification (the principle of permanent control). However, this principle must not be interpreted as meaning that certainty can only be approached but not reached in empirical science. Certainty is—as we have emphasized—the mode of validity peculiar to analytic propositions. Hence we cannot interpret a high degree of probability as an approximation to certainty or absolute truth.

This is seen even more clearly if we consider that the weight of a proposition is relative to a given scientific situation. Truth and falsity, on the other hand, the two truth-values of classical logic, were supposed in traditional logic to have no relation to the process of inquiry. We have rejected this view and have pointed out that deductive logic is concerned exclusively with propositional meanings regardless of the validity of the propositions. But whether or not we accept the traditional interpretation of 'truth' and 'falsity,' we cannot regard any logical calculus as both a generalization of two-valued Aristotelian logic and as a structural scheme of empirical procedure.

We have seen in this chapter that three different meanings of 'probability' must be distinguished. The first meaning relates to synthetic propositions as such, the second to undecided propositions with respect to given evidence, the third to relative frequencies in large series of events (statistical laws). Probability preferences (in accordance with the second meaning) are often

determined by statistical laws, but they cannot be deduced from such laws. Specific procedural rules are presupposed in the transition from statistical laws to 'probability preferences.' Probability laws in statistical theory are a specific type of rules of procedure relating to predictions of relative frequencies in large series of events. Some of the chief difficulties in the analysis of the probability concept have their root in an inadequate conception of 'truth' and in the failure to distinguish clearly between deductive logic in the strict sense and the logical rules of empirical procedure. The notion of probability is clarified by determining its place within the framework of these rules.

IN this chapter we shall show how failure to distinguish properly between matters of fact and relations of meanings has beclouded discussions of the relation between inanimate and animate nature and of that between psychical and physical fact.

The first issue is at the heart of the vitalist controversy. The basic question is: Are vital phenomena *sui generis* or are they merely highly complex physico-chemical phenomena? Vitalist doctrines affirm the former alternative, mechanist theories the latter.

A modern vitalist would not deny that physical and chemical processes play an important part in explaining the phenomena of life. He would admit that studies of the optical structure of the eye, of the leverage of the bones, of diffusion and osmosis, and of the chemical composition of organic compounds have contributed to the explanation of vital processes. But he would declare that these investigations do not suffice for the explanation of such processes. To substantiate this claim, various types of arguments have been advanced by vitalists. We shall first refer to the older and now superseded arguments.<sup>1</sup>

The first argument concerns the complexity of organic compounds. During the early decades of the nineteenth century it was asserted that chemists would never be able to produce organic matter artificially (synthetically). A special vital force was thought to be required, an agent the creation of which is beyond human capacity. This theory was refuted when Wöhler

produced synthetic urea. Soon other organic substances, like haemin, a main constituent of haemoglobin, were produced synthetically.

Nevertheless, vitalists did not admit defeat even though they had to make suitable modifications of their thesis. They now argued that the fact of synthetic production of organic substances in the chemical laboratory was to be conceded, but that the manner in which these syntheses occurred was entirely different from their production in nature. The classic example adduced was the transformation of carbon dioxide into sugar, of particular importance in plant life. From the fact that yeast was indispensable in this process it was concluded that it contained the vital force required to start the process. But this assumption also proved untenable. Zymase, isolated from dead yeast, is able to bring about fermentation. It is now established that catalytic effects are involved, and, although the role of catalytic agents in chemistry is in need of further explanation, every chemist knows that catalytic processes are by no means confined to the vital sphere.

Besides this first (*biochemical*) group of vitalists, there is a second group that supports its thesis by reference to the *specific mode of functioning* of living bodies or their constituents, cells. They stress nutrition, growth, reproduction, and response to stimuli. In opposing them the mechanists tried to provide mechanical analogues of these processes. To this end, great ingenuity was exercised in the construction of 'artificial cells,' hardly distinguishable from natural cells with respect to observable movements.

The analogies, to be sure, cannot be carried very far, but they are not devoid of significance, since they caution the vitalist against hastily claiming certain kinds of phenomena as peculiar to life processes. As a matter of fact, vitalistic arguments of this type recede more and more into the background, since they are readily refuted by the mechanists. Rather than specific simple phenomena, a peculiar *nexus* of phenomena that—according to the vitalist view—can be accounted for only *teleologically* is now considered essential for life processes. The outstanding repre-

sentative of this neo-vitalist tendency—which may be called *psycho-vitalism*—is H. Driesch.<sup>2</sup> His arguments for the autonomy of life now form an essential part of vitalist theory.

Of these, the principle of 'organic regulation' may be mentioned first. Driesch showed, by experiments on the eggs of sea urchins, that fragments of widely differing form and size were able to regenerate complete and typical organisms. This seemed to provide a significant example of teleological regularity, for the initial states might vary to a great extent and yet the developmental idea 'residing' in the sea urchin always led to the same result. On the other hand, Driesch argued, we cannot conceive of a machine that, after small parts had been removed, could restore its previous form and function by utilizing surrounding materials.

Driesch viewed his 'proof from the genesis of equipotential systems' (heredity) as a second conclusive argument for the autonomy of life. He considered it absurd to suppose a machine capable of dividing itself into several parts from each of which the whole could restore itself. Hence we must assume regulative forces guiding biological processes in the direction of the predetermined end.

It has been declared a weak point in Driesch's argument that the question whether physics and chemistry suffice for explaining the phenomena of life is treated in terms of the contrast between organisms and machines. This stricture is justified, though Driesch's formulation of the issue follows the traditional pattern set by Descartes and the materialists of the eighteenth and nineteenth centuries. But there are more incisive arguments against Driesch's position. To show that a nexus of phenomena of the kind that Driesch regards as peculiar to life is also found in inanimate nature, his opponents referred to mobile equilibria in physical chemistry—to systems that, despite continual changes in the relative positions of particles, constantly retain a particular state of equilibrium and assume a new state of equilibrium after the removal of particles. It was also emphasized that such systems, e.g. suspensions, maintain the relative positions of their



parts after innumerable divisions, and that liquid crystals, which fuse together in restoration of their original form, may be taken as the counterpart to cellular division.

This brief survey should suffice to give some notion of the facts introduced by vitalists in support of their thesis. However, we are interested here not in these facts for their own sake but in the methodological conclusions drawn from them by the vitalists.

It is characteristic not only of Driesch's reasoning but also of that of most other vitalists that the alleged impossibility of explaining organic by inorganic processes is attributed to their essential heterogeneity. Here we have an instance of the deep-rooted belief that the effect must resemble the cause, a belief that has one of its sources in the confusion of relations of meanings with matters of fact. To make this point clear, we shall analyze the question: Are life processes explainable in terms of the laws of physics and chemistry?

Explanation of a fact in terms of empirical laws consists in deducing the synthetic proposition asserting that fact from accepted synthetic propositions. If we now say that life processes (biological facts) cannot be explained in terms of physical or chemical laws, we may mean two things, namely: (a) that life processes cannot be explained in terms of laws *at present* belonging to physics or chemistry but that they may be explained in terms of such laws established at some future date; or (b) that such explanation is impossible regardless of the progress of science.<sup>3</sup> The first thesis is hardly contested nowadays by opponents of vitalism. But it is different with thesis (b), the central thesis of vitalism.

A question of possibility or impossibility (in the strict sense of these terms) is not one of fact; it is a logical question. If a proposition  $p_3$  contains a term not contained in either of two given propositions,  $p_1$  and  $p_2$ , then it is impossible to deduce  $p_3$  from  $p_1$  and  $p_2$ . Explanation of the phenomena of life in terms of physical phenomena is accordingly (logically) impossible if the term 'life' is not reducible to the terms of physics. The

essential point is thus the definition of 'life.' Anti-vitalists hold that 'life' can be defined in terms of physics; vitalists claim that an additional element of meaning is required in 'real' definitions of 'life' and consequently of all other biological terms.

But the discussion has been beclouded by the erroneous view, common to both parties, that the settlement of this issue would prejudge the question whether living beings can arise out of inanimate matter, which is obviously a question of fact. Let us suppose that analysis of the concept 'living being' has led to the conclusion that 'living being' cannot be defined exclusively in terms of physics, while, on the other hand, it has been established by experiment that living organisms of a peculiar kind can be produced by combining particular chemical elements in a specific way. The pertinent empirical law would be: 'Wherever and whenever the elements  $e_1, e_2, \dots e_n$  are combined in these proportions, living beings will come into existence.' The mechanist will interpret this result as a physical explanation of life, since it is based exclusively on physical facts; the vitalist will interpret it as a non-physical explanation, since it is in terms of a law containing the (supposedly) non-physical term 'living being.' But it is obvious that the empirical significance of such an explanation is not affected by a resolution to accept one rather than the other of these interpretations.

To avoid complicating the issue, we have referred only to empirical laws in our analysis. But the crucial point of the argument is not touched by the inclusion of theoretical laws. We then have explanations in terms of rules of procedure, and these rules must contain the biological terms of the propositions to be explained.

Misunderstandings of a similar kind are also associated with the thesis that inquiry into the origin of life will disclose the 'nature' of life. There are the conflicting views that life is as old as the earth and that it arose on the earth by spontaneous generation out of inorganic matter. The vitalists hold that living organisms are intrinsically different from inanimate objects, and therefore cannot spring from them, whereas their opponents con-

test this. But throughout the discussion there is a continual shift from meaning to genesis, and conversely. These remarks apply also to the doctrine of emergent evolution.

The principal difference between living organisms and inanimate matter is, according to the vitalists, that a purposive factor—*élan vital* (Bergson), *entelechy* (Driesch, following Aristotle), *dominants* (Reinke)—is operative in the processes of life. Accordingly, they claim that reference to such a factor must be made in the definition of 'life' and that teleological methods are the proper methods of biology.

Of course, they do not dispute the fact that biological processes, such as assimilation, fertilization, reproduction, and growth occur in space and time and may therefore be considered morphologically. But, according to the vitalist thesis, the morphological approach does not enable us to comprehend the peculiar nature of the phenomena of life. Nor does the causal method of physics suffice for an explanation of life processes, since life is governed by purposes. Only a functional approach that emphasizes the purpose of each process and the contribution made by the different organs is appropriate. In defining 'biological processes' or 'organs of living beings' exclusively in terms of physics, we do not grasp their essence.

Let us now examine this view. He who declares that all vital processes are purposive seeks to draw analogies between them and purposive human actions, finding in the latter the prototype of purposive process. Here an end is 'given' (or 'set') if there is present a human resolve to bring about some events or prevent their occurrence. The execution of this resolve is called the attainment of the end.

It is misleading to contrast causal with teleological (functional) approaches, since teleology implies causality. The analysis of purposive behavior discloses two relations that may be called 'causal': (1) the relation between the intention to perform and the performance of actions that are supposed to 'lead to the end'; (2) the relation between these actions and the occurrence of the desired facts. Failure to distinguish between 'end-

in-view' and 'real end' is responsible for the erroneous view that the temporal relation of cause and effect is reversed in the means-end relation and that the end is the 'creator' of the means. Applied to the issue of vitalism, this view seems to provide an ultimate justification of the vitalists' claim that assumptions concerning the purposes of biological processes are indispensable in biological explanations.

Is this interpretation of biological inquiry adequate? Is there indeed reference to non-physical facts in investigating the 'function' of a certain organ or the 'end' of a certain organic process—reference such as is made in psychology or the social sciences when we operate with the concept of purpose? In these sciences we find propositions of the form: 'Certain persons have set themselves certain ends.' These are synthetic propositions about psychological facts and are subject to empirical control. Given a particular purpose and particular actions, we may ask: (a) whether the actions are conducive to the attainment of the end, and (b) whether they were performed for the sake of the end.

Now the functional method in biology is concerned exclusively with questions of type (a). Questions of type (b), which may be called questions of *subjective rationality*, play no role here. If it be said, nevertheless, that certain processes occur as if produced by planned activity, this 'as if' is to be considered merely as a challenge to exhibit the common characteristics of biological processes not involving conscious acts and purposive human actions. The basis of the analogy is that in both cases a set of facts is considered under the aspect of their 'causal contribution' to the production of a particular effect and moreover that certain types of changes in the causal factors leave the effect unaltered.

The second point may require further clarification. If a person is unable to perform a particular action meant to play a part in the promotion of a given end, one will seek to replace him with a substitute, i.e. a person that will equally well promote the given end. Thus, for example, a builder who has given a task to one worker in the construction of a house will entrust it to

another worker if the first falls ill. We say in this case that the second worker has the same *function* as the first, or that his work serves the same purpose. But in examining whether the second worker actually accomplished the task assigned to the first, we need not refer to the fact that this result was desired by the builder.

The interpretation of the term 'function' is similar when we say that after removal of an organ—e.g. a kidney or a part of the cortex of the brain—another organ—the other kidney or another part of the brain—assumed the functions of the organ removed. We need not invoke 'purposes of nature' to describe this fact. We establish merely that two series of events, different in certain respects, are equivalent with regard to particular effects, e.g. the preservation of a certain kind of organism. Since practical interests associated with the study of vital phenomena—e.g. in horticulture, animal breeding, and medicine—are directed primarily toward the conservation and propagation of living beings, there is a tendency to concentrate upon these phenomena and to classify other phenomena with respect to their causal relations to the former. This is the nucleus of the functional approach. It leads to the introduction of functional rather than morphological terms in biology in accordance with the tendency to establish a terminology appropriate for a chosen method of inquiry. But this should not induce us to attempt an ultimate justification of the functional approach in biology by claiming that it is predetermined by the essence of life, which is governed by purposes of nature.

It follows that propositions of biology apparently containing teleological concepts can be formulated without using teleological terms. Thus, for example, the proposition 'The inclination of the two optical axes serves for binocular vision' could be formulated: 'Without inclination of the two optical axes (*ceteris paribus*) binocular vision would not be possible.'

Our criticism is not directed against the functional method, but only against the claim that its appropriateness can be proved *a priori*. For example, in declaring that there is no reference to

ends in the concept of 'organic function,' as the term is used in biological inquiry, we do not deny the possibility of acquiring important knowledge of the structure of an organ by studying its functions. Nor do we mean to deny that 'organism' should be defined as a system of functionally interrelated components.

Analysis of this concept shows that it cannot be defined solely in terms of a set of morphological traits. Reference must be made to interactions implying laws. Accordingly, we must distinguish between 'organism' and 'body' if we understand by 'body' the morphological substratum of the organism. We cannot speak of parts of the organism in the same sense as we do of parts of the body. A unity defined in terms of *interactions* is different from a morphological unity. When we say that an organism is composed of organs, we mean that parts of the body interact in accordance with particular laws. That an organism is more than the 'sum' (aggregate) of its organs thus means that 'organism' is not understood as a morphological term, but as the '*field of application of biological laws.*' However, terms of physical science, such as 'electromagnetic field,' are not morphological terms either. It is impossible, therefore, to differentiate between biological and physical objects in terms of the distinction between 'wholes' and 'aggregates.'

The foregoing analysis of the meaning of 'purpose' as related to biological phenomena applies in particular to the idea of an ultimate end supposed to govern all vital processes, namely, *the preservation of the species*. Sentences of the form 'A particular kind of behavior of plants or animals serves the purpose of preserving their species' assert nothing else than the proposition 'Without such behavior their species would be more likely to perish.' Such an assertion can be tested in various ways, but none of these controls involves reference to 'purposes of nature.'

Discussion of the so-called *psycho-physical problem* has been infected with no less ambiguity than has the vitalist controversy by the confusion of analysis of meanings with issues of fact. The question whether psychological concepts can be defined in terms of physical concepts or vice versa is continually confused

with that of the causal relations of physical facts; and the 'principle' that the effect must be contained in the cause plays an important part here too.<sup>4</sup> Four fundamental positions with respect to the psycho-physical problem result from the two theses, 'The physical and the psychical differ essentially' and 'The essentially different cannot affect each other,' together with their respective antitheses.

1. The physical and the psychical differ essentially and do not interact (theory of psycho-physical parallelism).

2. The physical and the psychical differ essentially but nevertheless interact (theory of psycho-physical causality).

3. The physical and the psychical do not differ essentially, because the psychical can be reduced to the physical (monistic materialism).

4. The physical and the psychical do not differ essentially, because the physical can be reduced to the psychical (spiritualism).

Usually only theses 1 and 2 are contrasted as opposite views of this problem, but materialism and spiritualism also propose specific (monistic) solutions by denying the heterogeneity of physical and psychical facts.

Dualists as well as monists, in dealing with this problem, usually confound relations of meanings with causal relations. The dualists manifest this confusion in the very formulation of the problem. For only if we set out from the assumption that things must be 'essentially' similar to be causally related does the causal relation between the physical and the psychical require philosophical clarification. Furthermore, only on this assumption does conflict arise between the theories of psycho-physical causality and psycho-physical parallelism.

Descartes' treatment of the psycho-physical problem, which initiated its discussion in modern philosophy, has not been free from the confusion just described. He first inferred from the fundamental diversity between *substantia extensa* and *substantia cogitans* that neither could affect the other. But then he found himself induced by empirical facts to envisage an *influxus*

*physicus* localized in the pineal gland.<sup>5</sup> The inconsistency was trenchantly criticized by the occasionalists (Geulincx and Malebranche<sup>6</sup>). They are the founders of the doctrine of parallelism, also adopted (with some qualification) by Leibniz.<sup>7</sup>

Nor have monistic doctrines avoided that error. They attempted to prove that there is no fundamental heterogeneity between physical and psychical phenomena by stressing the causal relation between them.

Methodological analysis leads to the settlement of the controversy. Both 'interaction' and 'parallelism' refer to the correlation between psychical phenomena and physical phenomena in terms of given laws. Whether these laws state the coexistence or the succession of the phenomena to which they relate is not of fundamental importance.

Analysis of psychological concepts is more difficult than that of physical concepts. Much has been accomplished in this direction during the past seventy years, particularly by Franz Brentano, William James, and Edmund Husserl, but we cannot yet say that we possess a systematic classification of psychical facts. It is hardly questioned that the old tripartite division into thinking, willing, and feeling, which largely determined the structure of Kant's philosophical system, will have to be modified. But agreement among psychologists on this issue does not extend much further. Failure to distinguish properly between internal relations of meanings and causal relations has continually impeded progress. This applies particularly to the interpretation of emotions as disturbances of the soul by the influence of the body. This 'explanation' of emotion, which has been at the core of rationalist ethics since antiquity, implicitly contains the result of an analysis of meanings according to which the only purely psychological concept is that of thought, whereas the emotions are psycho-physical, i.e. phenomena to be defined in terms of both spheres.

It should be sufficiently clear from the foregoing that to call man a psycho-physical unity is not to say that he is an 'aggregate of a body and a soul. Yet this view has not only dominated



mythical thinking, but it still works its way into subtle philosophical speculations about the psycho-physical problem. Unless we emancipate ourselves from it, we shall not be able to formulate clearly the philosophical problem of the understanding of fellow men. This issue cannot be treated here in detail, but one point of major importance for the methodology of the social sciences must be mentioned.

We cannot 'perceive' men and human actions in the same sense in which, say, we 'perceive' a red disk. Perceptions of the latter kind also involve syntheses, but additional syntheses on a higher level appear in the former. Certain data of observation are interpreted as symptoms of the existence of people at certain places or of the presence of human actions. For example, observation of the behavior of sellers and buyers in a market is not just observation of the movements of physical bodies in space. Rather, the observed physical facts are correlated with psycho-physical facts. Three different interpretations of this process have been offered, and here again the two issues of the *origin* of beliefs of a certain kind and of the *validity* of these beliefs were seldom clearly separated.

1. All objective knowledge of behavior of others is knowledge of observable facts and their causal connections with other facts of the physical world, including the human body. This view has found its most elaborate presentation in modern behaviorism.

2. In addition to immediate experience of external objects through sense and of our own behavior through self-observation (introspection), there is also immediate apprehension of other people and their actions. This view has received its most elaborate formulation in the writings of Max Scheler.<sup>8</sup>

3. Knowledge of the behavior of others is knowledge by analogy. We infer from observable physical facts (particularly bodily movements), the occurrence of psychical processes in our fellow men similar to those that we have found (by introspection) in ourselves as accompanying physical facts of the same kind. This is the prevailing view.

The first interpretation is refuted by an analysis of meanings

by which it is made clear that psycho-physical terms are not reducible to physical terms, or, in other words, that psycho-physical facts are different in kind from physical facts. We shall return to this point in Chapter XI. The second interpretation is exposed to the general objection to any theory of immediate knowledge. The third interpretation is not fundamentally wrong, but it oversimplifies the problem of making explicit the implicitly acknowledged rules of procedure in terms of which control of propositions about the psycho-physical world is defined. And this is precisely the point that is significant for the methodology of the social sciences.

The chief difference between the rules of procedure concerning propositions about the psycho-physical world, and those concerning propositions about the physical world is that the protocol propositions are of a different kind. In the psycho-physical domain they imply interpretations by which psycho-physical facts are correlated with physical facts. But the two kinds of protocol propositions have an essentially similar status in scientific procedure. Both can be sufficient conditions for the acceptance or elimination of singular propositions. This is one reason why they are seldom properly distinguished. Another reason is that particular sets of observational data are often 'automatically' interpreted as psycho-physical phenomena.

Besides protocol propositions about the behavior and psychic states of others, protocol propositions about our own behavior and psychic states are basic in psychology and the social sciences. In distinction from the natural sciences there are, then, two different types of protocol propositions here. Protocol propositions reporting self-observations cannot be replaced by the protocol propositions of other persons. There is but one person who can make a report (protocol) of self-observation concerning the state or behavior of person P, namely, P himself. This is the only case in which the observer cannot be taken as 'anonymous' in the rules of scientific procedure.

The preference rules of procedure concerning propositions about the psycho-physical world are not substantially different

from those concerning propositions about the physical world. The ideals of unity, simplicity, universality, and precision are in both fields regulative principles of inquiry. But approximation to the ideals is less close here than in natural science.

Now that we have distinguished between propositions about physical facts and propositions about psycho-physical facts, the question arises whether the whole field of meanings has thereby been encompassed. This might be denied on the ground that value judgments imply peculiar fundamental meanings. But the analysis in the following chapter will show that they do not.

COMPARISON between epistemological doctrines and ethical or esthetic doctrines discloses a parallelism of theses and supporting arguments that cannot be explained simply by pointing to the tendency to harmonize the three main classes of human ideals, the True, the Good, and the Beautiful. There is, rather, a far-reaching conformity in the logical structure of the problems. To show this conformity and thereby to clarify the meaning of value judgments is the primary purpose of the present chapter. As an introduction we shall offer a synopsis—necessarily incomplete—of these parallelisms, restricting ourselves to the comparison of epistemological and ethical doctrines.

The role of sensations in epistemology corresponds to the role of feelings in value theory. According to naive realism, both are produced by things-in-themselves, in that the human body and, indirectly, the human soul are 'affected' by them. On this view the moral values 'good' and 'bad' are properties of persons or actions just as sensible qualities like size, shape, and color are properties of physical bodies. The former, like the latter, are 'immediately given.' This interpretation was contested by the Sophists. They disputed both the objective validity of fact-statements based on sense perceptions and the objective validity of value judgments based on feelings of love or hate, appetite or fear, although some Sophists, like Protagoras, shrank from applying their skeptical views to ethical and political issues. Frequently, they pointed to 'sensory illusions' and argued that

since indubitable truth cannot be attributed even to the apparently self-evident data of sense perception, it is still less justifiable to make this claim for ethical judgments. The result of the Sophistic criticism was, accordingly, that certainty is unattainable in both cases.

Far from contesting the unreliability of the testimony of sense, the polemic of the Socratics against the Sophists strongly emphasized it. The Socratics declared, however, that there is a higher kind of knowledge, that 'true' being and 'true' value can be grasped by pure reason with absolute certainty. Reason teaches us what we must correctly accept as true or reject as false and also what we ought rightly to love or hate, approve or disapprove. In Plato's theory of Ideas logic (ontology) and ethics fuse into a unity. The concept is considered as an ideal, and a value hierarchy corresponds to the logical hierarchy of concepts.

Reason—theoretical as well as practical—being viewed as infallible, it becomes necessary to explain the 'origin' of error and sin. The usual explanation is that man is not a purely rational being, that he is burdened with a body, that this body, the 'prison of the soul,'<sup>1</sup> the seat of sense and of passion, produces the disturbances called errors or sins. This theory, nourished by Oriental mystical sources, was wrought by the Neo-Platonists into a great speculative system, which, with some modification, was adopted by the Schoolmen, and substantially influenced the great rationalists of the seventeenth and eighteenth centuries. We have already, in Chapter I, referred to the various aspects of this view and may here amplify the list of opposites mentioned there: activity—passivity, form—matter, soul (mind, reason, spirit)—body, infinite—finite, truths of reason—truths of fact, judgments *a priori*—judgments *a posteriori*, good—bad, freedom—bondage, virtue—vice (sin), happiness—unhappiness. On the rationalist view, errors are manifold, but moral as well as theoretical truth is one, and human reason is an image of the Divine Reason to the extent to which it can comprehend Divine Reason—the one truth.

The psychologistic interpretation of logic and ethics, according

to which feelings of evidence are criteria of truth, arose within the bosom of rationalism in spite of the fact that it is incompatible with the fundamentals of the rationalistic doctrine. Accordingly, the use of the terms 'intuition' and 'clear and distinct thinking' in the history of philosophy is ambiguous. If these terms are taken to be applicable exclusively to the apprehension of meanings, then their use is in accordance with the rationalistic doctrine; but this is not the case if intuition or clear and distinct thinking is taken to be the criterion of the truth of synthetic propositions. In modern philosophy we find this ambiguity already in the epistemology of Descartes,<sup>2</sup> and it becomes still more apparent with the sensationalists. But Leibniz emphasized that clear and distinct thinking is possible only of propositions the truth of which rests exclusively upon the principle of contradiction, i.e. of analytic propositions (*verités de raison*). Here, as in many other respects, Leibniz was far ahead of his time. Even today we still meet with the attempt to prove the truth of synthetic propositions by referring to feelings of evidence.

The chief difference between rationalistic subjectivism and empiricist subjectivism (psychologism) in the interpretation of the 'objectivity' of truth is that the consensus of men with regard to fundamental truths is declared by the former to be a metaphysical, by the latter an empirical, fact. The former find the source of these truths in reason, implanted by God in all men; the latter, in the fact of similar somatic constitution. Acceptance of the psychologistic view suggests investigations to discover how far consensus extends and to what factors we may attribute disagreement among individuals or groups. Such differences of opinion are even more conspicuous with respect to value judgments, and reference to conflicting views in this field has always provided arguments in support of philosophical relativism. It is generally assumed that factors of inheritance, tradition, milieu, and interest have a great influence on the formation of human beliefs and attitudes, and thus the question arises whether other judgments too are influenced by these factors. An answer in the affirmative is then regarded as proof that truth 'depends upon'

the conditions mentioned above and is co-variant with them. In this way the problem of the *criteria of validity* not only of value judgments but also of value-free judgments is confounded with the problem of the *origin* of valuations and opinions.

After the publication of Darwin's *Origin of Species*, we find a strong tendency to seek a biological foundation for ethics as well as epistemology and to define both the good and the true in terms of biological concepts. Many attempts have been made to overcome the dualism of truth and value as well as of theory and practice by tracing them back to a common biological origin. All these investigations concerning the genesis of valuation must be strictly separated from the logical analysis of value judgments, to which we now turn.

In the first place, we have to differentiate between value judgments and propositions asserting the occurrence of emotional acts like loving, hating, desiring, fearing. That there is indeed a difference in meaning is recognized when we consider propositions such as the following: 'Mr. N is loved by all who know him, but he is not worthy of their love.' The first part of this proposition is an assertion about the occurrence of some emotional acts. The second part is the negation of a corresponding value judgment. Now, clearly the compound proposition is not self-contradictory. Its second part does not negate what is affirmed in the first part. Rather, it criticizes the psychical acts the existence of which the first part asserts.

The same distinction applies to psychical acts that are more properly called valuations, e.g. approving, disapproving, preferring. It corresponds to the distinction between a proposition asserting an actual belief and the judgment that this belief is correct. For instance, we may correlate the following two propositions: 'N.N. believes that he will get this position, but his belief is not correct (warranted)' and 'N.N. prefers his present position to the one he had before, but it is not preferable (worthy of being preferred).' <sup>3</sup>

'Correct belief' is defined in terms of rules of scientific procedure; 'correct preference' in terms of rules of valuation (*axio-*

*logical rules*). The sentence 'A certain belief of a person N.N. is correct' is elliptical, and the same holds for the sentence 'A particular preference of N.N. is correct.' In stating, for instance, 'Mercy killing is immoral' we presuppose axiological rules in terms of which 'immoral behavior' is defined. He who makes such a statement, and claims for it objective validity, is bound to substantiate it by referring to the criteria of immoral behavior.

We thus recognize a parallelism between methodology and axiology. Valuations as well as scientific decisions are choices, and with respect to both, the question of correctness in terms of implicitly presupposed rules may be raised. The (elliptically formulated) value judgment 'A certain valuation is correct' corresponds to the (elliptically formulated) methodological judgment 'A certain scientific decision is correct.' Judgments of both kinds are *analytic propositions*. It is essential for an understanding of value judgments to realize first that to assign a positive or negative value to objects of a given kind is to declare that a certain valuation of these objects is correct in terms of presupposed axiological rules, and secondly that, when completely formulated, the judgments are analytic. We have emphasized in Chapter IV that the logic of scientific procedure is not concerned with *actual* scientific decisions in *actual* scientific situations, but with decisions of a certain kind in situations of a certain kind, and that judgments of correctness are obtained by analysis of the rules of scientific procedure. In applying such judgments to actual scientific decisions in actual scientific situations, we presuppose that their kind is determined.

Similar considerations apply to value judgments. These judgments are obtained by analysis of axiological rules; they refer to any objects of given kinds. In applying a value judgment to an individual object, we presuppose that this object is of the kind to which the value judgment refers, i.e. we presuppose the validity of a particular synthetic proposition. But this does not affect the status of the value judgment proper as an analytic proposition. To regard value judgments as synthetic propositions



and to contrast them with value-free synthetic propositions is the *proton pseudos* in value philosophy.

We shall say that values are assigned to objects *by virtue of* their properties (including relations), but this does not imply that values are simply given with the properties without reference to a set of axiological rules. This latter view is an error similar to the one discussed in Chapter VI, where we emphasized that, contrary to prevailing opinion, a warranted prediction need not be deducible from accepted synthetic propositions, but may be related to them by rules of empirical procedure (theoretical laws).

We are now able to appraise the controversy whether value judgments are objective or subjective. Different issues are usually confounded in the discussion. Value judgments may be called 'objective' in the sense that they are not concerned with subjective preferences but with correct (rational, warranted) preferences, all these words being synonyms, defined in terms of presupposed axiological rules. However, they are not 'objective' in the sense of being deducible from propositions asserting properties of objects (either qualities in the strict sense, or relations to other objects) irrespective of rules that relate these properties to human preferences. Only *in terms of such rules* are values 'constituted' by those properties.

Failure to recognize this is at the root of the doctrine (endorsed in contemporary philosophy by G. E. Moore) that values are intrinsic properties of objects.<sup>4</sup> Closely associated with the correspondence theory of truth, it usually claims that there is immediate experience of values. It follows from our analysis that this view is untenable.

This issue has usually been confounded with the issue whether value judgments are objectively valid in the sense that general consensus is attainable with respect to them, as it is for value-free statements, particularly in the natural sciences. The subjectivists have maintained that this is obviously not the case, that vast differences in value standards have been found by ethnological studies and explained in terms of hereditary and environ-

mental factors. They have pointed out that value standards are substantially altered when the economic and political structure of a society changes, and that it is but prejudice to claim general validity for a particular system of values, our own system, which is in most cases the system of the religious or political group or the social class to which we belong.

In examining the significance of these arguments for the theory of value, we have to distinguish between

- (a) the typical preferences (valuations) of men,
- (b) the standards by which these preferences are actually judged as right (correct) or wrong (incorrect),
- (c) the standards that are arrived at by rectification of the actual standards.

Divergences in actual preferences are no proof of differences in value standards. Most persons do not always behave in accordance with the standards by which they judge other people's behavior, just as they do not always choose to believe or disbelieve in conformity with the standards of scientific inquiry implicitly acknowledged by them. (Kant's categorical imperative demands that they should behave in accordance with these standards. *Moral will* is the will to do so.)

There are undoubtedly great differences in the standards, too, but this should not lead to the hasty conclusion that there are no invariant properties characteristic of all systems of moral rules. To assume this would be no less erroneous than to suppose that the rules of procedure characteristic of different groups of sciences, such as the natural and the social sciences, have nothing in common.

An ethnologist who studies a primitive people is soon able to understand their mores even if at first he does not understand their language. This would hardly be feasible if he could not presuppose fundamental similarities between the mores of this people and of other peoples well known to him.

Such invariant properties concern the loyalty of a person to his group (or groups), his readiness to co-operate in matters

important to the group, and to comply with regulations (tabus) prevailing in the group. This involves a certain degree of benevolence toward other members of the group, veracity in giving information, and reliability in keeping pledges.

All these elements in the standards of moral evaluation may, at a certain stage of development, relate exclusively to the attitude of one member of a particular group to fellow members. However, as soon as social relations other than unrestrained warfare are established between different groups, a wider field of application (though not without modifications) is required for them. With growing knowledge the tendency is strengthened to make all mankind the field of application of these rules,<sup>5</sup> excluding (partially or completely) only those who are regarded as enemies of one's own group. This extension of ethical rules is one particular way of rectifying them, linked generally with realization of the basic similarity of all men. Other types of rectification are concerned with the elimination of inconsistencies between different moral rules and their integration into a system in the frame of which their relative rank is determined. Determination of the rank of a moral rule is established by definition of 'correct preferences' in case of moral conflicts. Our remarks in Chapter III on the meaning of rectification of rules of method apply to the rectification of axiological rules. We assume in either case that with increasing knowledge and insight a person will modify hitherto accepted rules with a view toward integrating them into a coherent whole. The rationalists supposed that this process of rectification would result in perfect consensus about the meaning of goodness (real happiness) and beauty. This claim cannot be upheld, but the amount of implicit consensus should not be underrated either.

At any rate, determination of the amount of consensus concerning the rules of correct valuation is not a problem of value logic (axiology). In passing a value judgment, we declare that a certain valuation is correct in terms of a particular system of rules presupposed as given. A discussion about whether an action  $a_1$  is morally preferable to an action  $a_2$  between a Benthamist

and a philosopher who accepts Kant's or Nietzsche's conception of morality is bound to lead to confusion if the participants fail to make clear what they mean by moral preferability. The philosopher N might be ready to accept the thesis of the philosopher P that the action  $a_1$  is morally preferable to the action  $a_2$  in terms of P's definition of moral preferability and still oppose P's thesis as he understands it, since he defines 'moral preferability' in a different way.

Nevertheless, it would be superficial to dispose of such controversies as insignificant terminological differences. The definitions of ethical terms that a man proposes may be indicative of his personal preferences and prospective actions. We trust that a man defining 'moral action' in terms of the promotion of the happiness of the greatest number will be more likely to strive for an improvement of the standard of living of the working classes than will a man who defines 'moral action' in terms of the promotion of the emergence of the superman.

Considering this, one might be tempted to say that a moral judgment pronounced by a man is simply a declaration of his attitude and of his intention to induce others to adopt a similar attitude. However, this interpretation is not adequate. The content of a value judgment does not entail any reference to the man who makes it. If, in a discussion of value judgments, there is consensus concerning the presupposed axiological rules, it will be the aim of the discussion to establish whether the judgments at issue are correct in terms of these rules. But even if the axiological rules accepted by the participants are at variance, the discussion need not be futile. In the first place, it can be examined whether the value judgment made by each of the participants is in accord with his own axiological rules. A second and more important topic of such a discussion would be a reconsideration of the proposed axiological rules with a view toward determining whether they are 'real' definitions, i.e. whether they are in accordance with the fundamental principles of valuation implicitly acknowledged by the proponent of these rules. The result may be their rectification; the modified rules will correspond to a

higher level of clarity. To be of aid in attaining clarity is the aim of the Socratic method. Socrates does not aim at persuading his partner in the discussion to accept a moral judgment inconsistent with his (the partner's) moral principles. Rather, Socrates tries to make him aware of those principles and then to show that the judgment at issue is in harmony with them. This, incidentally, marks the distinction between higher moral education and indoctrination of moral beliefs.

We shall give an example.

Many people are prone to regard as unfair any actions of their fellow men that conflict with their own interests. But if they possess a certain degree of intelligence, it can be made clear to them, so long as their emotions are not aroused, that they would have evaluated an action of this sort differently had it not conflicted with their interests, and that they cannot expect their condemnation to be shared by a 'disinterested observer.' They will then see that a definition of 'unfair action' according to which every action conflicting with their interests is to be called 'unfair' would not correspond to what they 'really' mean by 'unfair action.'

Let us summarize our argument. By a value judgment the valuation of a given object is declared to be correct in terms of presupposed axiological rules by virtue of the properties of the object. Value judgments are analytic propositions. The erroneous view that they are synthetic propositions has its root in the elliptical formulation of value judgments and value problems. The sentence: 'A certain action  $a_1$  is morally good' is elliptical. Its complete formulation is ' $a_1$  is morally good, in terms of a system of axiological rules, by virtue of its having the properties  $p_1, p_2, \dots p_n$ .' If the presupposed synthetic propositions asserting that  $a_1$  has the properties  $p_1, p_2, \dots p_n$  are confounded with the value judgment proper, then it seems as if the value judgment itself were synthetic.

We must therefore distinguish carefully between the two questions: 'Has  $a_1$  the properties by virtue of which it is to be called morally good in accordance with given axiological rules?' and

'Should  $a_1$  be called morally good in terms of these rules by virtue of its properties?' The answer to the first question is a value-free synthetic proposition, the answer to the second is a value judgment, an analytic proposition.

Failure to realize that value judgments are analytic propositions suggests that there is a specific realm of values, which is the object of value judgments, as the real world is of factual statements. Our objection to this view is not that it differentiates between value judgments and statements of fact but that it claims for values a peculiar ontological status. We shall discuss this point more thoroughly in Chapter xv.

## PART II

### Methodological Issues in Social Science





IN recent years the term 'crisis' has frequently been applied to the state of science in general or of particular sciences and groups of sciences. There has been a 'crisis' in physics, a 'crisis' in psychology, and above all a 'crisis' in the social sciences—sociology, economics, jurisprudence, etc. The term refers in the first place to the rise of doubts concerning laws and methods that had previously been regarded as firmly established. But as applied to the social sciences, it is, furthermore, indicative of a profound dissatisfaction with the results of social inquiry. We are constantly reminded of how much the natural sciences have contributed to the promotion of human welfare and how little the social sciences have accomplished in this respect. To the argument that the social sciences cannot be held responsible for social practice it is replied that their results are too vague to afford a sound basis for the solution of practical issues. The demand is made that they serve toward the regulation of social life, just as the natural sciences, particularly physics, have served toward the technological mastery of the forces of nature. Thus, the social scientist is inclined to draw the conclusion that the method of physics, if applicable at all to the social sciences, is the only correct or at any rate the best method.

The primary question is, then, whether this method can be applied in the social sciences. And here, as so often happens, exaggeration of the contrast between divergent views makes it difficult to penetrate to the roots of the issues at stake. An addi-

tional obstacle has been the lack of clarity sometimes found among social scientists concerning the meaning of physical laws.

Doctrines demanding the adoption of the methods of natural science in social science will be called 'naturalistic,' and those that reject this demand will be called 'anti-naturalistic.' We shall give a brief synopsis of the arguments pro and con on this issue.

According to the naturalistic thesis, inquiry in the social sciences can be called 'scientific' only if it is conducted in accordance with the methods of the natural sciences, particularly physics. In so far as these methods are applied, the social sciences fall within the domain of natural science and are accordingly not autonomous sciences.

This thesis is supported by the following arguments: Only the methods of the natural sciences do full justice to the austere demands of scientific research. They alone have led to precise and relatively simple laws that permit reliable predictions covering wide ranges of time and space. These laws order themselves into a hierarchical system; and if the ideal of a completely unified and all-inclusive system has not yet been achieved, there still remains the hope that even the gap between the laws of classical physics and the laws of quantum physics will finally be bridged. All assertions in natural science are intersubjectively controllable, many of them by experiment. And the mathematical form in which they appear is a token of their precision.

In comparison, the social sciences seem to be in a bad way. This is partly explained by the fact that their subject matter is more complicated than that of the natural sciences. But the methods employed in social science are also responsible. These methods are, with few exceptions, still in a stage of development long since passed by the natural sciences. Social scientists mistrust abstraction, are reluctant to substitute quantities for qualities, and accordingly operate with data that are not intersubjectively controllable. Hence the social sciences cannot exhibit such hierarchy of laws as appears in physics, and the language of social science does not have the clarity required in scientific re-

search. The ambiguity of the nuclear terms, such as 'society,' 'economy,' 'law,' 'state,' leads to unceasing controversies about their 'true' meaning. Such deficiencies can be remedied only by reconstructing social science on the model of natural science. To be sure, this reconstruction will meet with great difficulties. The method of experiment will not be so widely applicable as in physics, but statistical inquiries will provide a kind of substitute. In the social sciences, too, we must concentrate on mathematical relations of phenomena. Instead of operating with mental activities and dispositions, we shall have to deal with observable events susceptible of measurement.'

There are two major groups of anti-naturalists. Common to them is the view that the method of physics is inapplicable in social science. They diverge, however, on the issue of laws. According to the first group social science contains strict laws *sur generis*. The second group, on the contrary, maintains that there are no strict laws at all in social science.

The first group points to the procedures of introspection and understanding and declares that these are specific methods of social science, leading to specific social laws. These laws are frequently regarded as superior to physical laws because 'they flow from sources that lie in ourselves.'

The theses of the second group, which disputes the existence of genuine laws in the domain of social science, are based primarily on the following arguments:

1. Human freedom of will introduces a factor of indeterminacy into social prognoses, so that no social law can be expected to hold without exception. For the decisions of men are largely influenced by irrational factors that defy calculation.

2. Experiment plays an essential part in the discovery of physical laws. But in the social sphere experiments are possible only on such a moderate scale that it would be inappropriate to base a method on them.

3. Physical laws are valid for all places and times. All assertions in social science, on the contrary, refer to specific historical circumstances.

4. So-called social laws lack objective validity. They vary with the 'perspective' of the social scientist, particularly with his temporal distance from the events to be explained, with his social setting, and his personal equation. Moreover, social science is not value-free, as is natural science.

5. The precision of physical laws is due to their mathematical form, but mathematics is not applicable to the social sphere. For numerical comparability requires measurable magnitudes, and this condition is not fulfilled here. This argument is also accepted by many social scientists who assert that the laws of social science are *sui generis*.

The question of the relation between natural and social science is so important in methodological controversies within the social sciences that it can be made the central point of methodological analysis in this field. It even pervades methodological problems that appear, at first sight, to be unaffected by it, as, for example, the problem of the relation between psychology and the social sciences. Scientists who insist upon a psychological foundation of social science are usually inclined to think of a particular psychological method as the 'true' method; and here again we find the contrast between naturalism and anti-naturalism. The following remarks taken from an article by C. L. Hull show that one can hardly speak of *the* method of psychology:

One of the most striking things about the present state of the theory of learning and of psychological theory in general is the wide disagreement among individual psychologists. Perhaps the most impressive single manifestation of the extent of this disagreement is contained in 'Psychologies of 1925 (14)' and 'Psychologies of 1930 (15).' In these works we find earnestly defending themselves against a world of enemies, a hormic psychology, an act psychology, a functional psychology, a structural psychology, a Gestalt psychology, a reflexology psychology, a behavioristic psychology, a response psychology, a dynamic psychology, a factor psychology, a psycho-analytic psychology, and a psychology of dialectical materialism—at least a dozen.<sup>1</sup>

Generally speaking, we cannot assume that there is only one method of inquiry into a given subject matter. Unfortunately, this assumption is implicit in many methodological discussions in the social sciences. Thus, it is debated whether, for social science in general or for a particular social science, e.g. economics, 'the' physical method or 'the' biological method or 'the' psychological method is 'the' adequate method. The resulting vagueness in stating the problems is enhanced by other factors. It is often hard to tell whether a methodological postulate expresses (a) an ideal that inquiry may only gradually approximate, or (b) a heuristic program, to be realized immediately, which involves the abandonment of all actually employed methods at variance with it, or (c) the demand for correct interpretation of methods already employed.

Example of (a): The postulate that the laws of the social sciences, like those of physics, contain only numerical relations of measurable magnitudes. It is admitted that this goal will not soon be reached, for which reason we must be content at present with less strict laws, with 'mere rules.' But the postulate stands as a guiding principle of research.

Example of (b): The postulate that the method of Understanding be established as the fundamental method of social science, so that the social sciences are to contain no law referring exclusively to physical facts.

Example of (c): The postulate that social science be considered as a branch of psychology, supported by the argument that social scientists have been prevented from realizing this by the preconception that sociological terms like 'society,' 'state,' 'law,' etc. are irreducible to psychological terms.

No little confusion has resulted from introducing into social science a method supposedly identical with one that had proved successful in natural science, although in fact the two methods were similar only in certain respects. As a consequence, social inquiry was led astray by false analogies. The 'organic' theory of society and the state is a case in point. If, therefore, the adoption of a method is at issue, it will not suffice to characterize it ac-

cording to its source. The method must rather be defined in terms of its specific traits. The belief that it is an acknowledged method of a particular science may then prove to be erroneous. If so, we must not argue in its favor that it has already been successful in that science.

Moreover, the demand that a method of some other science be adopted in a social science frequently fails to indicate how far the application of that method is to extend. Sometimes the science adopting the method loses its autonomy and becomes a branch of another science. But this need not be the case.

Another factor responsible for persistent methodological controversies is the failure to realize that each of two apparently conflicting methods may have its proper place in the investigation of a particular subject matter and may yield significant results denied to the other. In illustration we may refer to the question whether the history of an art should be treated in terms of its *specific* problems or whether it should rather be related to the comprehensive category of 'mind' or 'spirit' of a particular period.

According to those who adopt the first approach, e.g. Heinrich Wölfflin, Gothic architecture, for instance, is, after all, architecture. Gothic architects, like those of any other period, were faced with the peculiar problems of architecture. These problems had to be solved under specific conditions, including knowledge, capacity, the desires of patrons, available materials, etc. And it is the specific mode of solution, under such conditions, that gave Gothic architecture its specific character. Hence our task as historians of art is to investigate these problems and conditions and to show how they determined the particular expression of the general idea of architecture during this period. In contrast, those who adopt the second approach, e.g. Jacob Burckhardt, conceive the problem as follows: Gothic architecture is a creation of the Gothic man. In order to understand it we must, therefore, first understand the Gothic man. We cannot isolate the specific manifestations of his spirit as they present themselves in architecture. We must have a synoptic view of manifestations of every avail-

able variety in order to penetrate to the core of his personality. Only when this is grasped can we really understand his attitudes and values and, in particular, the manifestations of the Gothic spirit in architecture.

Each of these approaches leads to significant results for the history of art. It is futile to ask which is the more significant as long as we do not indicate spheres of theoretical or practical interests in regard to which the concept of 'significance' acquires a definite meaning. It can perhaps be said that the former method is more significant for the art critic whereas the latter is more significant for the philosopher. But the essential point is that in deciding which of two methods is to be preferred in a given context of inquiry, one first has to determine the structure of each in order to realize what they have in common and in what respects they differ. Such an analysis, to be thorough, could not stop short of the general methodological principles of empirical science.

In the following chapters we shall be primarily concerned with methodological difficulties relating to the general issues discussed in Part I. In particular we shall show how the fallacy of confounding analytic and synthetic propositions pervades many methodological controversies in social science.

The problems to be discussed are selected on the basis of the following considerations: (1) Their relevance for social science; (2) their relation to general methodology; (3) their bearing on the fundamental problem of the relation between the methods of natural science and those of social science. The controversy between behaviorism and introspectionism, to be dealt with in the next chapter, is significant from all these aspects.

THE basic tenet of modern behaviorism is that psychological and sociological inquiry must confine itself, in its descriptions and explanations, to observable facts and hence primarily to the bodily behavior of the objects it studies. The method of introspection (self-observation), which has occupied a central position in traditional psychology, is rejected as unscientific, on the ground that its results cannot be tested intersubjectively. Defenders of the method of introspection reply that human activity can never be understood if we conceive of human beings simply as physical objects, and that the behaviorists will never be able to work in accordance with the methodological principles they profess. They would always have to make implicit use of the results of introspection, in violation of their own postulates.

This discussion shows particularly well how controversies about scientific methods are affected by the attempt of each party to offer an ultimate justification of his view. Since we shall confine ourselves primarily to an analysis of such justifications, we shall not find it necessary to differentiate between the different varieties of behaviorism and introspectionism.<sup>1</sup> It will suffice to contrast the fundamentals of the two doctrines and to show that the attempts to justify them on ultimate grounds rest on presuppositions which we have proved in the first part of this book to be untenable.

The behaviorist who does not venture to offer an ultimate justification of his method but contents himself with supporting it



by reference to established experience will argue as follows: The method of the introspectionist consists in asking the subject about his experiences in particular experimental situations and taking his reports as data, subject to certain restrictions relating primarily to the reliability of the subject. But this method suffers from obvious deficiencies. In reflecting on our own experiences, we unavoidably 'rationalize' and hence distort them. The type of distortion varies with the type of experience, the personal equation of the subject, and the environmental conditions. Predictions concerning the behavior of people on the basis of what they say about themselves are frequently inaccurate. On the other hand, experience has shown that systematic observation of animals and infants, guided by biological and physiological hypotheses, leads to reliable predictions of their behavior. We have good reason to assume that inquiry based on similar principles is preferable to asking people about their experiences.

An introspectionist who is free from aprioristic preconceptions will be ready to admit that the practice of asking subjects about their psychical states can easily lead to errors if the subject and the experimental conditions are not carefully controlled. He will even concede that, in this respect, much has been neglected. Nevertheless, he will argue, the proposal to give up the method, to refuse to avail ourselves of self-observation, would be like the proposal to abandon the method of observation in physics because of the possibility of sensory illusion. What we ought to demand, in both cases, is that the most reliable system of controls be instituted. The methods of animal and child psychology have indeed been successful, but they could not have achieved what they did if the inquiries in which they were employed had not been guided by hypotheses of an obviously teleological character. But hypotheses of this sort are based on the results of introspection. Behavioristic method is thus by no means independent of introspection.

If the issue is formulated in this way, the prospect seems favorable for a settlement of the controversy, with the beneficial consequences for scientific progress that are to be expected from

a clearer understanding of the relations between the two methods. But the outlook is not so bright when each of the doctrines claims a monopoly for its method and bases its claim on supposedly *a priori* grounds. And, in fact, both behaviorists and introspectionists have frequently taken such a stand.

The chief aprioristic argument of the introspectionists appears in the thesis that psychical facts, by their very nature, can be grasped only by self-observation or understanding. The latter is interpreted either as an inference by analogy from the ego to an alter ego, or as intuitive 'feeling into,' 'feeling at one with.' Often the results of introspection, and also those of the understanding of others, are declared to be absolutely certain. We can, it is said, only register and classify causal connections in the physical world, without being capable of insight into their final grounds, whereas we are able to have an immediate experience of the concatenation of psychical acts, and to comprehend them 'from within.' Although knowledge based on introspection is contrasted here with knowledge based on sense perception, there is, nevertheless, a far-reaching parallelism between this view and sensationalism. Both doctrines are intuitionistic. They hold in common the belief that a single, isolated act can provide ultimately valid knowledge of fact and that all mediate knowledge derives its validity from immediate knowledge. We have shown in Part I that this view is untenable, that the validity of propositions about psychical or psycho-physical facts, like that of propositions about physical facts, is determined by a potentially endless process of inquiry, in which propositions are accepted or eliminated on the basis of accepted propositions. To be sure, there are very difficult problems involved in the analysis of the validity of protocol propositions, but these problems are not at issue here. The decisive point is that propositions incorporated into science on the basis of protocol propositions are not exempt from elimination by later controls.

Lack of clarity on this point is more dangerous in psychology and social science than in natural science. An indefinite number of people may observe the same physical fact, but a psychical

fact can be observed by one person only. 'Protocols' about a person's psychical states can stem only from himself. (This is the meaning of Münsterberg's definition that the psychical is what is given only to *one*.)<sup>2</sup> If, now, we start with the erroneous thesis that all knowledge is derived from immediate experience, we are likely to draw the conclusion that assertions of a person about his own experiences are not susceptible of intersubjective control. This leads to the repudiation of methods that aim at systematic observation and causal explanation of manifestations of psychical states in the external world.

The behaviorist's critique of the introspective method likewise starts with the presupposition that the psychical is given only to one, but he derives from it different methodological conclusions. The behaviorist points out that it is erroneous to assume a realm of objectively existing psychical facts beside the objectively existing realm of physical facts. What is only subjectively given cannot form the object of scientific inquiry, for which intersubjective control is essential. This critique has received its most elaborate formulation in physicalism, a component of the doctrine of logical empiricism.<sup>3</sup>

Although the original formulation of physicalism has been modified in recent years, particularly by Carnap, it is by no means devoid of interest. For it reveals the logical core of the argument of radical behaviorism, which is still quite influential. Moreover, the fact that this eminent logician has considerably modified his arguments for behaviorism is not without significance. It is therefore worth while to examine briefly the early and present stages of physicalism as exemplified in the writings of Carnap and his disciple, C. G. Hempel.

The basic thesis of earlier physicalism is that every confirmable sentence of psychology is translatable into a sentence of physics. This thesis can be divided into the two assertions: that every sentence of psychology is translatable into a sentence about spatio-temporal events, and that every sentence about the latter is translatable into a sentence of physics. Only the former assertion need concern us here.

The physicalistic argument rests on an analysis of meaning according to which the meaning of a sentence is established by its truth-conditions, which, in turn, point back to 'control sentences.' Carnap defines the term 'control sentence' as follows:

Given a synthetic sentence (i.e., a sentence about facts) called E, two cases can be distinguished, without, of course, wishing hereby to draw excessively sharp boundaries between them. In the first case E can be controlled by direct experience. In the second case we have to control E indirectly, in that we derive from E and other—scientifically accepted—sentences certain directly controllable sentences (control sentences for E).<sup>4</sup>

Accordingly a sentence is meaningless if it is unconfirmable in principle by observation. Two sentences have the same meaning if and only if they are true, or false, under the same conditions.

As an illustration we shall give an example taken from an apt presentation of early physicalism by Hempel.<sup>5</sup>

*I. Physical sentence:*

The temperature of the physics laboratory was 23.4° C today at such and such a place.

Examples of control sentences for this sentence:

A mercury thermometer with a Celsius scale shows a coincidence between the mercury level and the line 23.4.

An alcohol thermometer shows another precisely established coincidence.

The pointer of a galvanometer connected to a thermocouple makes a certain swing if the element (couple) is brought to that place at the time indicated.

*II. Psychological sentence:*

Paul has a toothache.

Examples of control sentences for this sentence:

(a) Paul cries and makes gestures of such and such a kind.

(b) Asked 'What ails you?' Paul utters the words 'I have a toothache.'

(c) Further investigation reveals a carious molar with exposed pulp.

(d) Paul's blood pressure, his digestive processes, his reaction times show such and such changes.

(e) Changes of such and such a kind occur in Paul's central nervous system.

The argument, in summary, is as follows: All control sentences for any sentence of psychology are sentences containing only terms that denote spatio-temporal objects and properties. Thus, any sentence of psychology is an abbreviated formulation of a set of sentences about the physical world. Reference to introspection is not permissible in a truly scientific psychology, for introspection is not objectively confirmable, and only objective terms are permitted in a science.

There are two objections to this argument: first, that the meaning of a sentence is not identical with its truth-conditions, and second, that not all the truth-conditions for a sentence are control sentences in Carnap's sense. We pointed out in Part I that the meanings of the sentences to be verified in science are presupposed in the process of verifying them. Propositional meanings are logically prior to the rules of verification relating to these propositions (truth-conditions). The second objection is implied in our criticism of immediate experience. The thesis that all control of sentences can be reduced to direct control is derivable from the sensationalist doctrine of immediate experience, which we have shown to be untenable. Carnap was influenced by this doctrine in the form that Wittgenstein gave it in his theory of atomic propositions, and the logical empiricists have emancipated themselves from its influence only gradually. There are still some remnants of the earlier view in the recent reformulation of their argument.

We have emphasized in Chapter VIII that the role of protocol propositions reporting self-observations in the control of assertions about psychical facts is like that of protocol propositions reporting sense observations in the control of propositions about physical facts. Moreover, we have pointed out that protocol propositions are not exempt from control. A protocol proposition reporting a person's self-observation can be called 'more subjective' than one reporting a sense perception if this is taken to mean only that no person can directly experience another per-

son's psychic state. But *indirect* intersubjective control is not excluded. Protocol propositions about self-observations may be checked in many ways, particularly by interpreting bodily symptoms. The argument that psychology and the social sciences must not make use of reports of self-observations because the latter are 'merely subjective' is therefore untenable. Such reports have their proper and by no means insignificant place within a framework of controls.

However, the decisive objection to the older physicalistic doctrine is that it fails to distinguish clearly between issues exclusively related to the meaning of psychological terms and issues concerning the verification of psychological propositions. Identifying meaning and truth-conditions, the physicalist claims that the repudiation of introspection as a scientific method logically implies the exclusion of all psychological sentences that cannot be translated into the language of physics.

In his recent revision of the physicalistic thesis Carnap no longer holds that intersubjectively controllable sentences about the psychical states of a person are nothing but statements about physical facts. The following quotations from his *Logical Foundations of the Unity of Science* are representative of his present view.<sup>6</sup>

In psychology, as we find it today, there is, besides the physiological and the behavioristic approach, the so-called *introspective method*. The questions as to its validity, limits, and necessity are still more unclear and in need of further discussion than the analogous questions with respect to the two other methods. Much of what has been said about it, especially by philosophers, may be looked at with some suspicion. But the facts themselves to which the term 'introspection' is meant to refer will scarcely be denied by anybody, e.g., the fact that a person sometimes knows that he is angry without applying any of those procedures which another person would have to apply, i.e., without looking with the help of a physiological instrument at his nervous system or looking at the play of his facial muscles . . .<sup>7</sup>

Anger is not the same as the movements by which an angry

organism reacts to the conditions in his environment, just as the state of being electrically charged is not the same as the process of attracting other bodies. In both cases that state sometimes occurs without these events which are observable from outside; they are consequences of the state according to certain laws and may therefore under suitable circumstances be taken as symptoms for it; but they are not identical with it . . .<sup>8</sup>

We have to do with psychological terms not with kinds of events. For any such term, say, 'Q,' the psychological language contains a statement form applying that term, e.g., 'The person . . . is at the time . . . in the state Q.' Then the utterance by speaking or writing of the statement 'I am now (or: I was yesterday) in the state Q,' is (under suitable circumstances, e.g., as to reliability, etc.) an observable symptom for the state Q. Hence there cannot be a term in the psychological language, taken as an intersubjective language for mutual communication, which designates a kind of state or event without any behavioristic symptom. Therefore, there is a behavioristic method of determination for any term of the psychological language. Hence every such term is reducible to those of the thing-language.<sup>9</sup>

It is obvious that the standpoint of radical physicalism is abandoned here. Psychological terms are no longer taken to be *identical* in meaning with terms of the thing-language. It is only maintained that the behavioristic method permits the *reduction* of psychological terms to terms of the thing-language. This thesis is based on Carnap's theory of reduction statements.

The concept of 'reduction statements' is defined as follows: 'If . . . a certain term *x* is such that the conditions for its application (as used in the language of science) can be formulated with the help of the terms *y*, *z*, etc., we call such a formulation a *reduction statement* for *x* in terms of *y*, *z*, etc., and we call *x* reducible to *y*, *z*, etc.'<sup>10</sup>

Now it is apparent from the context of Carnap's *Logical Foundations of the Unity of Science* (as well as from his Harvard lectures on *Testability and Meaning*, where the term 'reduction statement' occurs for the first time) that one has to understand by 'conditions for the application of a term' conditions for the

acceptance or elimination of propositions containing this term. The preceding quotation concerning anger and its observable symptoms makes this point clear. There it is explicitly stated that the observable events regarded as consequences of anger, and therefore, under suitable circumstances, taken as symptoms for it, are deemed to be such consequences in terms of given laws. The laws that Carnap thus presupposes permit the transition from introspective to physical data. It is these very laws that warrant the substitution of the physical term (introduced by the reduction sentence) for the psychological term. But the laws contain the very term that is to be eliminated. Such terms are therefore not eliminated from science by the process of reduction. However, the possibility and desirability of this elimination is exactly the point made by radical behaviorists and physicalists, who tried to prove that psychological facts are 'really' physical facts. The attempt was doomed to failure.

However, as we have already emphasized, this is no objection to the behaviorist's program of inquiry, rightly understood, but rather to inadequate interpretation of it. By making its implicit presuppositions explicit, we bring to light the relation of this method to other methods of psychological inquiry. Similar remarks hold for intuitionistic interpretations of the methods of self-observation and understanding. Here it is particularly important to separate logical analysis of rules of procedure from psychological description of the cognitive process. From the fact that understanding often occurs with lightning rapidity and already at early stages of mental development we must not conclude that it possesses a simple logical structure. Failure to discriminate between genetic and logical priority has obscured discussions concerning the nature of understanding, particularly the issue whether, in understanding the behavior of our fellow men, we draw analogies with our self-observations (Ch. VIII). The question essential for the logical analysis of understanding is not 'How does the belief arise that certain fellow men find themselves in a particular psychical state or perform particular psychical acts at a given time?' It is rather 'What are the criteria



for a *warranted* belief of this kind?' And this question implicitly refers to given rules of scientific procedure.

In the present chapter we have dealt with a problem related to the meaning of psychological terms in the strict sense. The following chapter will be concerned with the meaning of sociological terms.

MANY doctrinal controversies in the social sciences have been complicated by the view that a particular approach to social problems can be ultimately justified by showing that the nature of social objects requires it. Insight into this 'nature' will, it is supposed, reveal the adequate method of social research and determine the appropriate practice in the corresponding fields of social action. This view has dominated discussion of the nature of social collectivities from the very outset.

For more than 2000 years the nature of the relation between social groups and institutions—particularly society and the state—and the individuals 'forming' them has been one of the pivotal issues of social philosophy. This problem becomes acute by reason of the following considerations: On the one hand, it seems clear that society is composed of a number of individuals and that, accordingly, no society can exist where there are not a number of individuals. Furthermore, when we say that society acts or is acted upon, we mean that there are individual persons who act or are acted upon. On the other hand, we say that a society can continue to exist if many or even all of its former members have been replaced by others. And we say, again, that the social spirit or mind, as it is manifested in language, customs, and political institutions, survives changes in the members of the society. Thus, the issue arises whether society, as a 'social whole,' is prior to the individual persons composing it or whether the opposite is true. This issue is frequently referred to as the

controversy between universalistic and individualistic conceptions of society.

If the approach to this problem is guided by the traditional ideas that the effect is contained in the cause and that the cause is more real and more valuable than the effect, confusion of genetic, logical, and axiological priority is bound to arise. One important instance is the attempt to determine the nature of society by tracing its origin to a social contract, which usually contains just those clauses that the author considers politically desirable. To be sure, some of the great social philosophers of the seventeenth and eighteenth centuries who had recourse to the idea of a social contract recognized that it is to be considered a fiction, serving as a regulative principle for social norms and institutions. But many teachers of natural law took it as historical fact and justified their political creed by appealing to this fact. Now the results of modern ethnology make it seem probable that primitive people generally lived in hordes, i.e. in a social state. However, we no longer believe that norms concerning the proper mode of social life can be derived from such knowledge. Indeed we can 'derive' opposite axiological conclusions from the same facts of primitive existence, depending upon whether, in accord with mythical traditions and with Rousseau, we extol the state of nature as ideal, or whether, taking our stand on ideas of progress and evolution, we view the primitive state as a starting point from which people ought to depart further and further.

We have seen that questions concerning the nature of things aim at 'real definitions' of terms, that is, of definitions in accordance with their 'rectified' use. But the use of sociological terms is by no means uniform. We are thus confronted with the task of determining of what differences in actually applied or postulated methods or in political goals the terminological differences at issue are indicative. And here we have to be equally on our guard against aprioristic dogmatism and radical conventionalism. We succumb to the former when we presuppose that every term actually used has only one 'true' meaning. We succumb to the

latter when we suppose that controversies about the nature or essence of objects or the 'true' meaning of concepts are 'merely' conflicts about words. This view disregards the fact that lack of clarity in language is due to lack of clarity in thought, and it tends to distract us from the task of determining the relations among the meanings of equivocal terms and of disclosing the problems of analysis 'behind' equivocations. Such an analysis will often reveal a core of meaning common to all equivocations. It will then be appropriate to establish a definition of the term by which it is given this nuclear meaning.

These considerations apply to the term 'society.' In order to disclose the core of its meaning, we shall start with the question: What does it mean to say that a number of people form a society? This phrase suggests that particular relations are established among these people, which may be called, in first approximation, relations of interaction. Indeed, Georg Simmel has defined society as a totality of individuals interacting with one another.<sup>1</sup> But this definition is too wide. Not every interaction between people, e.g. the casual crash of two cyclists, is called a social relationship. It is required, in addition, that the action of the people involved be influenced by the fact that they take account of the remembered or anticipated actions of other people. Max Weber has defined the terms 'social action'<sup>2</sup> and 'social relationship'<sup>3</sup> in this sense.

We shall call 'action' (*Handeln*) any human attitude or activity (*Verhalten*) (no matter whether involving external or internal acts, failure to act or passive acquiescence) if and in so far as the actor or actors associate a subjective meaning (*Sinn*) with it. Social action is 'such action as, according to its subjective meaning to the actor or actors, involves the attitudes and actions of others and is orientated to them in its course . . .' Social relationship is 'a state of attitudes (*Sichverhalten*) of a plurality of persons which, according to their subjective meaning, are mutually concerned with each other and orientated by virtue of this fact. The social relationship thus *consists* entirely and exclusively in the probability that there will in certain circum-

stances be social action of a meaningfully predictable sort, without reference to the grounds of this probability.'<sup>4</sup>

These definitions, as F. Sander<sup>5</sup> and A. Schütz<sup>6</sup> have shown, require certain modifications. Thus, on Weber's definition of social action, the intentional perception of a fellow man's action would have to be called 'social action,' but this would not be in accordance with the actual use of the term. But the fundamental results of Weber's analysis are not affected by this criticism nor were they meant to be.

We cannot discuss here all the important problems raised by Weber's definition of social relationship; we shall confine ourselves to one question that lies in the main direction of our analysis.

What is meant, we may ask, by saying that there is a probability that particular people will act in a particular way? The answer is that it is *warranted* to predict that they will act in this way. But such a statement is, as we have seen, an elliptical formulation, requiring completion by explicit reference to a law *in terms of which* the prediction is warranted. Considering further that such a law is not falsifiable by one negative instance, we realize that it is a theoretical law. Following Weber, we shall call such a law a *scheme of interpretation* (*Deutungsschema*) if it explains human action in terms of presupposed motives of the actor.

We may then introduce the term 'society' in the following way: The sentence, 'A set of people belong to a society of a particular kind' is to be synonymous with the sentence, 'A scheme of interpretation is established in terms of which social relationships of that kind among those people can be explained.' When we speak simply of 'society' without indicating its kind, we leave the kind of relationship undetermined.

To say that a society of a particular kind exists at a certain place and time means that there exists a field of application for a scheme of interpretation. It is to this scheme what an electric field is to the laws of electrodynamics. To say that a society

arises or disappears is to say that such a field of application arises or disappears. The same holds of states, legal orders, languages, and institutions of all kinds.

It will be appropriate to illustrate these formulations by the example of a game of cards (also referred to by Weber). The players form a society by engaging in a social activity that implies observation of certain rules, namely, the rules of the game. Each player pursues his own ends, which run counter to the ends of at least one other player and are more or less determined by the rules of the game and by the given situation (distribution of the cards, etc.). Consequently an observer can explain, and, to a certain extent, predict, the behavior of the players in terms of the rules of the game. Every card game is thus a field of application of rules.

The game begins when the activity of the players begins, and it ends when this activity ends. The people are actually card players in so far as their behavior is interpretable in terms of the rules of the game. These rules are invariant by definition with respect to all variations in the persons of the players and in the place and time of the game.

If the scheme of interpretation is taken as a standard (definition) of 'correct behavior' rather than as a principle for the explanation of actual behavior, then it is called a set of norms. We shall have something more to say about this 'normative aspect' of sociological definitions in what follows.

It may be objected to the definition of 'society' given above that it does not agree with the use of the term adopted when we say that a society acts or is acted upon. We must therefore show how the meaning of the term in these cases is related to the meaning established by our definition.

We speak of actions of a social collectivity if persons act who are selected in accordance with certain rules referred to in the scheme of interpretation, and if, moreover, these persons act in accordance with certain other rules contained in this scheme. Such persons are called 'organs' of the social collectivity. If the

organs *qua* organs are acted upon, we say that the social collectivity is acted upon.

Analysis of the concept of 'legal person' (such as a joint-stock company) will aid in clarifying this point. A legal person is 'represented' by its organs, but it remains the same even if all its organs change. Such a company may be sued and ordered to pay a debt regardless of changes in the personnel of its executives or stockholders. What actually occurs in such cases is that steps are taken against *individuals* who are selected in terms of legal rules as organs of the corporation. According to these rules it is unessential whether actions of a particular kind (e.g. the signing of certain documents) are performed by a person A or a person B, if both A and B satisfy certain requirements (e.g. are named or chosen by certain other persons for particular functions). The name *persona incerta* for 'legal person' in Roman law is suggestive of this point.

It has been hotly debated whether social collectivities are real objects existing independently of the persons who form them or whether they are merely mental constructs. We have shown in Chapter III the ambiguity of questions of this type and may therefore confine ourselves here to a few brief remarks.

It is easily seen that the issue does not concern matters of fact but relations of meanings. If we understand by 'independent existence of a social collectivity' only that we may have the same collectivity even if none of the persons who once formed it still belongs to it, then we must ascribe independent existence to social collectivities. But it should be borne in mind that a social collectivity 'exists' as a field of application of laws. It is 'real' in the same sense as is an electric field. The fact that the reality of an electrical field is physical reality whereas the reality of a society is psycho-physical reality does not affect the basis of the analogy. If, however, 'independent existence of a social collectivity' is taken to mean that the term 'social collectivity' is irreducible to terms denoting human relationships, then we must deny that social collectivities have 'independent existence.'

Universalists, beginning with the logical priority of society

over the individual, draw the methodological conclusion that we must not try to derive social phenomena from principles concerning the behavior of fictive 'isolated' individuals. They insist that society is not a 'sum of individuals' but a 'whole' and that we can understand the behavior of people in society only if we take into account their social *function*. They make frequent use of the analogy—which does not extend very far—between organism and society. This argument is correct in pointing out that in order to find adequate schemes of interpretation for social actions we must in most cases refer to given *ends* to which the behavior in question is supposed to be conducive. Corresponding to these ends is the 'public' interest, and this may conflict with other, 'private,' interests of the persons bound together by social relationships. If, however, society is hypostatized into a 'person of higher order'—as it is by universalists and organicists—then *a priori* reasoning is often thought to be capable of achieving 'the' just reconciliation between the public interest and the often conflicting private interests of the members of the society. And so ultimate justification of political doctrines is sought in the 'nature of society.'

The proponents of these doctrines argue that the value of a person is determined by his social function and by what he does in the performance of this function. Furthermore, these functions and accomplishments of people, together with their value, are conceived as determined by their knowledge, ability, character, or wealth. This view is frequently associated with the political demand that the rights of each person be 'proportional to his value,' a demand directed against the democratic principle of the equality of rights of all members of society.

On the other hand, the individualistic conception of society is likely to lead its proponents to a favorable attitude toward democracy. If less emphasis is placed on the social function of man, then it becomes plausible to claim 'natural equality' for all men by virtue of their constitutional similarity. This is then taken to be the basis of a similarity of vital needs, drives, and satisfactions, and stressing these will usually focus attention on



happiness-values. By arguing further that all men have an equal right to happiness, the proponents of the individualistic doctrine are easily led to accept the democratic creed. But emphasis on the material welfare of man is not necessarily associated with democratic principles. As the history of the eighteenth century shows, 'enlightened' absolutism has also aimed at maximizing material welfare.

However, it should be clear from the preceding analysis that we cannot ultimately justify a political creed by 'deducing' it from a particular interpretation of sociological terms. Although we may venture to explain the historical origin of the individualist and universalist doctrines as attempts toward ultimately justifying (conflicting) political creeds, such a justification is logically impossible.

It was the chief objective of the preceding observations to emphasize the distinction between logical analysis of sociological concepts and explanation and evaluation of social facts. In what follows we shall turn our attention to some difficulties in sociological analysis arising from the elliptical formulation of problems of the interpretation of social facts.

A problem in social science is not uniquely determined simply by setting the task of interpreting a particular social fact. The same fact may be interpreted in different ways, i.e. it may be regarded as a symptom of various kinds of facts. Furthermore, the interpretation of a particular fact may vary with the context in which it appears. In the complete formulation of a problem of interpretation the following points must be clearly stated:

1. What facts are to be interpreted?
2. What other facts can be introduced as data for this interpretation?
3. In terms of what laws (schemes of interpretation) should the interpretation be made?

In order to determine the relation of two different problems of interpretation, we must determine each of them with respect to each of these 'dimensions.'

Frequently, the nature of the object to be interpreted is not clearly enough indicated in the formulation of a problem of interpretation. For instance, the problem may be to interpret an inscription chiseled upon a stone in an Etruscan cemetery. It is then presupposed that the stone contains an inscription, i.e. symbols, and hence that the indentations were not caused, say, by atmospheric influences. Should the investigation lead to no results, we may decide that the problem was incorrectly posed, because the stone contains no inscription at all. The problem disappears with the abandonment of the fundamental interpretation of the indentations as inscriptions. We have emphasized in Chapter VIII that it is erroneous to conceive of human actions as observable physical facts, and this applies to artifacts and institutions. In all these instances there is indeed reference to the observation of physical facts, but we do not observe actions *qua* actions, artifacts *qua* artifacts, institutions *qua* institutions, and therefore we cannot say that they are 'given' in observation. Accordingly, we may state that every interpretation of social facts presupposes a fundamental interpretation, namely, that of the underlying physical fact as a social fact.

We shall further clarify this point by a brief analysis of the meaning of 'sign' and focus our attention upon the distinction between 'objective meaning' and 'subjective meaning' of signs. When we call a particular phenomenon a 'sign' (in the strict sense), we mean that it has been produced by someone in order to communicate something to someone else. It is possible to interpret signs without knowing who their communicator is. When we listen to an unfamiliar voice before our door or when we read an article without knowing who its author is, we find ourselves in this situation.

What happens is that a particular scheme of interpretation is applied to the acoustic or visual phenomenon, which is presupposed to be a sign of something. The objective meaning of a sign is its meaning according to a given scheme of interpretation adopted by a social group, e.g. the rules of the English language. Accordingly, it is elliptical to speak of the objective

meaning of a sign without indicating the implicitly presupposed scheme of interpretation. This elliptical formulation is associated with the erroneous view that the meaning belongs to the sign as a property to a substance.

Often the scheme of interpretation in terms of which the objective meaning of a sign is defined is regarded as a norm for the correct use of this term. For example, when we say that a certain person uses words incorrectly or makes a grammatical mistake, we mean that he has expressed himself in a way that departs from the rules of language. It is then usually assumed that the speaker intended to apply all the rules of the particular language but that he failed to comply with them. We should hardly say of a poet who enriches the language by introducing new words or phrases that he expressed himself incorrectly.

Whereas 'objective meaning of signs' refers to a presupposed conventional scheme of interpretation, 'subjective meaning of signs' refers to a scheme of interpretation adopted by a particular person, which implies reference to the person using the signs. Here the problem is to determine what meaning this particular person wanted to convey by the sign. The interpretation of a sign is called 'objective interpretation' if it aims at determining an objective meaning; it is called 'subjective interpretation' if it aims at determining a subjective meaning.

But even when this ellipticity in the use of 'objective' and 'subjective' meaning has been removed, the terms are not univocal. Failure to distinguish clearly between the proximate end in communicating a sign—the goal of conveying some knowledge to another person—and the further ends supposedly served by the communication (cf. Chapter II) is responsible for another equivocation. One of the further ends of the communication of a sign is often called the meaning of the sign, and the determination of this end is called the interpretation of the sign. In jurisprudence, where 'meaning of the law' is sometimes understood as 'meaning of the legal text' and sometimes as 'purpose of the law,' we must be on our guard against ambiguities of this kind.

In some types of problems of interpretation, for instance, in historiography, reference to all kinds of facts and laws is permitted. The historian shows his acumen in availing himself of a vast variety of data and general assumptions in the treatment of a given historical problem. On the other hand, in those social sciences that seek to assume the form of a unitary logical system, for instance, in economics, data and schemes of interpretation are restricted to a well circumscribed conceptual domain.

METHODOLOGICAL analysis of social laws has been largely concerned with the question why social science is not so successful as natural science in making predictions. We may distinguish objectivistic and subjectivistic interpretations of this fact. The former invoke an ultimate ontological contrast between the physical realm and the realm of human action. The physical realm is one of strict determinateness. Every physical event down to its most minute details is governed by a system of laws. But in dealing with human action, we must take into account a factor that is exempt from these laws, namely, free will.<sup>1</sup> Our inability to find social laws that are as strict and precise as physical laws is therefore not due to the feebleness of the human mind but has its source in the 'nature of things.'

According to the subjectivistic view there is perfect determination in both realms, but the laws relating to human behavior are more complex than those of the physical world and therefore have not yet been discovered to the same extent. Whereas the former view is indeterministic as far as the psycho-physical (including the social) realm is concerned, the latter is completely deterministic. But neither is genuinely scientific; neither offers controllable assertions. Causal determinateness may be said to have been established in a particular domain if all kinds of events occurring within this domain can be explained in terms of established laws; and if this could be done for phenomena in all domains, the causal determinateness of the world might be

said to have been established. But even then this causal determinateness could not be considered as permanently established, since at some future date some laws now established may have to be eliminated from science. If the claim is made that causal determinateness extends beyond the actually established laws, it is, as we have pointed out in Chapter VI, only the expression of the resolution not to give up the search for causes and of the belief that this search need not be in vain. On the other hand, the assertion that it is impossible to find such perfect laws in the social world as (supposedly) obtain in the physical world, since they do not 'exist' in the former, indicates a desire to justify on ultimate grounds the resolution not to search for them. However, it is impossible to find an ultimate justification for either resolution or indeed for any methodological resolution in an empirical science. This point is so important for the understanding of methodological controversies in the social sciences that we must dwell upon it a little longer. We shall offer a brief survey of the methodological resolutions that have been supposed to be deducible from, and thereby ultimately justified by, either the thesis of 'free will' or its opposite. This will make apparent the ambiguity of the term 'free will.'

THESES I. *The will is free, that is, incalculable.*

Methodological application: Since the will of man largely determines his social action, no social laws of the type of physical laws can be discovered. Hence, one should not try to discover them. Only certain rules or tendencies in social action can be found on the basis of careful observations interpreted with the aid of statistical theory.

THESES II. *The will is free, that is, not fully determined by external conditions.*

Methodological application: Explanation of social action and its effects cannot go beyond the psychical states of the persons acting. It is the teleological, not the causal, method that is appropriate for social science.

THESES III. *The will is free only under certain conditions,*

*namely, if the willing agent can operate undisturbed in the direction of the fixed goal (up to its realization).*

Methodological application: Only under these conditions is social action (and its effects) explainable by the psychical states of the actors. The factors considered as disturbances may be (a) physical events, (b) actions of others, (c) inner disturbances (errors, passions).

Accordingly, in explaining social actions, one should first try to determine the intention (the goal) of the actor. Then, in case the actions consequently performed do not lead to the projected goals, one must attempt to discover the various kinds of disturbances responsible for the deviation.

THESIS IV. *The will is free only conditionally, namely, in so far as the act of choice is exclusively governed by reason and is not affected by disturbing factors.*<sup>2</sup> (Formulated in this way, this thesis presupposes reason as a causal condition of an act of free choice. But it can also be understood as a definition of 'free will': a free will must satisfy the conditions in terms of which 'rational will' is defined. In other words, only a rational will is a free will.)

Methodological application: In order to understand social actions, we have first of all to determine what goals a prudent and well-informed man would have set for himself in the situation out of which the actions have arisen and what means he would have employed in the attainment of these goals. If there are discrepancies, then we must seek for factors to which the divergence of the will from a rational will may be imputed.

By linking the concepts of 'freedom' and 'rationality' with moral valuations, as rationalist ethics since Socrates has done, we arrive at further nuances in interpreting the concept of freedom of will. They derive from thesis iv by closer determination of the *content* of a will guided by reason alone.

THESIS V. *The will is free in so far as it is directed toward the highest good.*

Methodological application: In order to understand human

actions, we must determine the highest good. Hence, social science cannot be value-free.

THESES VI. *The will of man is free, which means, every person is responsible for his actions.*

Methodological application: The social sciences have to determine to what persons certain desired or undesired events may be imputed, thereby justifying public authority in rewarding or punishing those persons.

THESES VII. *The will of the normal, mentally healthy person is free, but not that of the mentally disordered.* Hence, only the former is responsible for his actions; they cannot be imputed to the latter.

Methodological application: Psychology must assume the task of establishing criteria of responsibility.

THESES VIII. *The will is not free.* If we knew the laws of physiology and biology as well as the laws of physics, we should be able to predict human decisions as accurately as we can predict natural phenomena.

Methodological application: The most important task of social science is that of discovering these laws. Therefore, it should follow in the path of natural science.

THESES IX. *The will is not free, but it is the resultant of a set of motives of different intensities.* Anyone who knew all the motives of a particular person in a particular situation and their intensities would be able to predict his choice with complete certainty.

Two arguments that lead to opposite methodological conclusions are related to this thesis. According to the first, men have similar basic needs, and hence similar basic drives, by virtue of their similarity in psycho-physical constitution. Social science should seek to establish laws of human action and social life related to those needs and drives that hold for all times and places. This argument suggests a close tie between social science and biology.

According to the second argument, there are typical drives, to be sure, but they vary so much with environmental conditions,



physical and social, that laws of choice, and hence of social action, can be found only for certain spatially and temporally restricted domains. Therefore, social science should beware of too broad abstractions and should rather concentrate upon well delimited historical phenomena.

It is obvious that 'freedom of will' is differently understood in theses I-IX. In order to explain how this variety of meanings of the term has come about, we should have to deal with the history of the problem, particularly with its status in medieval philosophy. Such a historical study would lead us too far afield. But one point within the scope of our main argument must still be mentioned, namely, that the fallacy of confounding synthetic with analytic propositions is partly responsible for the illusion that freedom of will is self-evident. If 'will' is defined as a psychological act that is the cause of an action of the person willing ('cause of an action' being understood as a sufficient condition for the occurrence of this action), it follows *per definitionem* that the will is free in the sense of 'efficient.' This, however, is not a synthetic proposition about the will, but the result of an analysis of the meaning of 'will.'

To penetrate to the methodological core of the deterministic and indeterministic views, we must refer to the distinction between empirical and theoretical laws. If the thesis that laws determine with necessity all events in the physical realm is taken to mean that we can make irrefutable predictions about physical events, then it must be rejected as incompatible with the principle of permanent control. But it can be interpreted in a different way, namely, as stating that physical laws can be found from which (in combination with accepted fact-statements) predictions can be deduced. This does not imply the claim that such knowledge is established once for all. Radical determinism concerning the physical world would then declare that predictions of all physical events can be made in this way, whereas a more moderate determinism would restrict this statement to particular classes of physical events. But the crucial question is: What does it mean to say that such laws can be found? We have already

suggested the answer, namely, that this proposition, apparently an assertion about the universe, is in fact the expression of the methodological resolution to search for such laws in the belief that the search need not be in vain. If we further consider that, according to the traditional view, predictions must be deducible from strict laws, i.e. laws not allowing for exceptions, we realize that the resolution corresponding to radical determinism demands that all predictions be in terms of empirical laws (synthetic universal propositions falsifiable by one negative instance).

We have emphasized in Chapter VI that this resolution is not adopted even in natural science. There we actually do make predictions the non-fulfilment of which is not supposed to have the elimination of a synthetic universal proposition as a logical consequence. Hence our distinction between empirical laws and theoretical laws. Whereas we have both types of laws in natural science, there are, as I see it, no empirical laws established in social science, and even the tendency to establish such laws is not very strong. But if we consider the significance of theoretical laws in natural science, we cannot regard this as constituting a fundamental difference between the methods of natural science and those of social science.

The thesis of radical determinism (concerning the physical world) is inadequate in another respect. Modern physics (even as it was before the rise of quantum physics) does not attempt to predict or explain everything, much less in terms of strict laws. For example, it does not seek to determine precisely the movement of every single molecule within a volume of gas. It confines itself to predicting the 'behavior' of the gas, which is taken to be the result of movements of a great number of such particles. It may be held that the aim of determining the movements of such particles has not been definitely renounced, but be this as it may, there is at present no procedure to this end even hinted at. Thus, we need not enter the psycho-physical realm in order to find unpredictable events.

One source of the belief that there are strict (empirical) laws

in social science may be found in the failure to distinguish clearly between such laws and 'rigid' theoretical laws (sometimes called conventions). A theoretical law may be called 'rigid' if there is no rule of higher order stating conditions under which it has to be eliminated. The principle of permanent control, which refers to synthetic propositions, does not exclude rigid conventions. Now it is readily seen that, in a sense, a rigid convention is radically different from a strict law. Whereas no control is established for the former, strict control is established for the latter. How, then, is it possible to confound one with the other? The confusion arises from operating with the conception of 'true' law.

The falsification of an empirical law is sometimes interpreted as follows: This proposition is not a true law; it was only erroneously held to be one. It is then impossible to falsify a 'true' law since a falsified proposition must by definition not be called a 'true' law. Thus, a 'true' law is regarded as immune to control like a rigid convention.

We shall offer an analogy in illustration. The legal rule 'The king can do no wrong' may be interpreted in two different ways. According to the first, whether a man is king is determined by a set of conditions not referring to the question whether he has done wrong, i.e. performed an action that would be called 'illegal' if one of his subjects had performed it. Any one of his subjects could be tried and punished for such an action, but not he. In other words, he is exempt from legal control. The other interpretation would be that a man who does wrong is not a king. In this case control of the actions of the man who is regarded as king is established and can even be exercised after his death. If the control discloses that he acted illegally, this will be considered as proof that he was not a 'true' king. The difference between these interpretations, one corresponding to rigid conventions and the other to strict laws, is obvious.

One is easily led to exaggerate the difference between the logical structure of the natural sciences (particularly physics) and that of the social sciences by comparing the procedure of the

latter, not with the procedure of the former as it actually is, but with a misinterpretation of it according to which all the laws of physical science are supposed to be strict laws.

This interpretation has been favored by the erroneous belief (criticized in Part I) that the mathematical form of laws is a guarantee of their truth. If it is then further held that, by the very nature of the subject-matter of social science, the laws of social science cannot have mathematical form, an ultimate ground is apparently provided for their not being genuine laws. Failure to distinguish clearly between pure and applied mathematics is chiefly responsible for this untenable view.

Applied mathematics is usually identified with measurement, and an essential difference between the methods of social science and natural science is often found in the fact that social phenomena are not measurable since they contain psychical elements.<sup>8</sup> We have pointed out in Chapter III the errors implicit in this view. It is based on the idea that the correct method of inquiry is uniquely determined by the nature of the objects under investigation. This idea again is closely linked with the preconception that giving grounds for assertions is in all instances tantamount to deducing them from synthetic propositions. In Chapter XVI we shall further illustrate this point by referring to a spirited controversy in economics in the past few decades concerning the question of the measurability of economic values (marginal utilities).

Aprioristic arguments for or against the appropriateness of mathematical methods in social science tend to divert attention from appraisal of the significance of such methods in particular contexts of social inquiry and to block the understanding of the real issues at stake. But it should be noted that the very employment of mathematical methods, which compels the scientist to formulate his problems more precisely, leads to clarification of implicit presuppositions and provides a corrective for such erroneous interpretations. In accomplishing this, the proponents of mathematical methods in the social sciences have substantially contributed to the understanding of scientific methods. The above

applies particularly to social statistics, which has developed magnificently in recent decades. The theory of representative sampling elaborated by statisticians is perhaps the most significant contemporary contribution toward the clarification of inductive inference.<sup>4</sup>

Another fundamental difference between natural and social laws is supposed to be exhibited in their respective relations to experiment. It is frequently maintained that the former are verified by experiments whereas the role of experiment in establishing social laws is regarded as almost negligible. This view requires modification in some respects. On the one hand, inquiry in natural science frequently has to be conducted with very little recourse to experiment, as, for example, in astronomy. On the other hand, it is often possible for the social scientist to experiment with single persons or social groups. Moreover, it should be borne in mind that it is not essential for the significance of observations made for the purpose of establishing laws whether they have been made by the use of particular technical apparatus, say, in a laboratory. Whether the conditions for systematic observation under the guidance of a theory have been artificially produced or simply found is not of primary importance. This is not to deny that the admirable experimental technique of the natural sciences has been a paramount influence in the development of these sciences. The preceding remarks are intended only to warn against a particular type of exaggeration of the disparity between natural and social science.

The argument just criticized is often combined with the two closely interrelated theses that the principle of causality, presupposed in all experiments, is not valid for the domain of psychological and social events, and that social phenomena, as historical, are unique, whereas potentially unrestricted repetition is presupposed in experiment. We have already dealt with the first of these two arguments and shall now examine the second.

Physical facts too are unique in the sense that they have a particular location in space and time. It may be said, however, that in looking upon a particular fact as a historical fact, we

stress its uniqueness. This point has been made by the Heidelberg school of Neo-Kantians. Rickert <sup>5</sup> (following Windelband <sup>6</sup>) distinguishes between generalizing (nomothetic) and individualizing (idiographic) sciences.

There is undoubtedly a methodologically significant point involved in this distinction. This point becomes obvious when one compares a textbook of theoretical physics with a textbook of the history of physics. In the textbook of theoretical physics the emphasis is on the laws, and the facts that are introduced as their experimental basis are not viewed as 'individual,' but rather as 'typical.' In a textbook of history, on the other hand, the emphasis is on single facts, and it is viewed as essential that they have occurred at a particular place at a particular time. The textbook of the history of physics tells what Galileo did in Pisa in the year 1591. The textbook of theoretical physics treats of experiments on which the law of falling bodies is based, without referring to the historical date of these experiments.

However, this distinction between theoretical physics and history (as Rickert clearly saw) cannot be derived from any difference in 'nature' between physical and social facts. Physical facts can be treated in an individualizing science and social facts in a generalizing science. Accordingly, Rickert grouped geology with history, and economics with physics. But it must be admitted that the generalizing approach is more successful in natural science than in social science. Usually a physical explanation or prediction is based upon a rather small number of facts, the type of which is well determined within the frame of a theoretical system. A larger number of factors must be taken into account if we are to explain or predict social facts, and the factors to be considered are not so well determined as they are in natural science. This difference between natural and social science is implicitly referred to in contrasting reproducible physical facts with unique historical events. But we must not regard it as a fundamental difference, derivable from the nature of the two kinds of facts. If we do so, we are prone to accept the thesis that historical science is purely descriptive, that there

are no historical laws. But this view is untenable. What can be rightly maintained is only that there are no *strict* (empirical) historical laws, falsifiable by a single negative instance. But historical science is not lacking in *theoretical* laws, i.e., rules of inference from given facts to other facts. Otherwise, no warranted prediction concerning the aims or consequences of human actions, no warranted appraisal of their significance, would be possible. The thesis that there are no laws in history may be explained in many instances as an extreme reaction to the emphasis on determinism in the philosophies of history of Hegel, Comte, Marx, Spencer, and Spengler.

Once these inadequate interpretations of the meaning of social laws are abandoned, the way is opened toward a more thorough analysis of the question: In what respects are physical laws 'more perfect' than social laws? This is a very involved problem, particularly by reason of the great multiplicity and complexity of social laws. We shall have to confine ourselves to a few brief suggestions. The comparison will be between theoretical laws in both fields; 'degree of perfection' is defined in terms of the ideals mentioned in Chapter VI.

### *The Ideal of Unity and Simplicity*

The hierarchy of social laws is less perfect than that of physical laws, but hierarchical structure is not completely absent. Implicit general assumptions about the physiological nature of man, such as his vital needs, his restricted life span, his biological development, his mental capacities and emotional reactions, are common to most social sciences. We should therefore beware of exaggerating the contrast between social laws and physical laws in this respect as well as in others mentioned before. Similar observations hold for the simplicity of social laws.

The fact that the principles of the social sciences are less 'unified' than those in the natural sciences is largely responsible for methodological particularism in the former. Since the social scientist has little reason to expect that principles of highest generality will afford the key to all the different predictions he

seeks, the problem of the choice of methods suitable from the point of view of their relevance for *specific* theoretical goals takes on far greater significance than it does for the natural scientist. Of course, the technician too has his specific theoretical goals suggested by practical goals as guiding principles for his research. But his approach is determined to a greater extent than that of the social scientist by laws of high generality, and his results occupy their predetermined place within a broader theoretical framework.

### *The Ideal of Unrestricted Universality*

We have already mentioned in Chapter v that the fundamental laws of physics are supposed to hold unrestrictedly in space and time whereas the fundamental laws of social science, being concerned with human behavior, are not meant to be of unrestricted universality since spatio-temporal limitations are implied in the concept of man.

One could, of course, suggest a definition of 'man' that would be free from spatio-temporal limitations and then speak of men who lived a thousand billion years ago on a planet of some ultragalactic star and to whom the fundamental social laws apply just as they do to people on the earth. We actually do conceive of physical objects in this way. For example, we do not define 'sodium' as a chemical element of such and such properties *that is present on the earth*, but we define it exclusively in terms of its properties. As a matter of fact, by spectrum analysis we can establish that sodium is present on a certain star and thereby control pertinent laws. It is conceivable that an analysis of the sodium spectrum of  $\alpha$  Centauri may lead to a revision of the present view concerning this element. But the control of the laws of human behavior by investigation of the behavior of transterrestrial rational beings is not only technically unfeasible; it is not even taken into consideration. This suggests that social laws are regarded as *restricted universal propositions*.

Accordingly, the task is set of making explicit the presupposed



range in space and time of the accepted laws of the different social sciences.

*The Ideal of Precision*

Social laws are, in general, less precise than physical laws. But this difference in degree of precision should not be interpreted as reflecting a fundamental methodological difference between social science and natural science, derivable from the mathematical form of physical laws. Many rather precise warranted predictions of events in the social world are continually made in daily life. We have a rather definite idea about what will happen to a letter that we mail or to a railway train for which we have purchased a ticket.

In general, predictions of a high degree of precision in the social sciences will have to be based on a much larger number of data than is required in natural science. Many highly precise predictions of events in the social world are warranted on the assumption that particular goals are given and that people act so as to attain these goals. We shall refer to this point again in dealing with value problems in social science (in Chapter xv). But the treatment of these problems requires the previous analysis of the 'objectivity' of social science, to which the following chapter is devoted.

*The Ideal of Pervasiveness*

We have already implicitly referred to this ideal in discussing the issue of determinism.

IN the last chapter we examined several attempts to provide ultimate justifications for methodological resolutions in the social sciences and found that opposite *a priori* principles have been invoked to support conflicting methodological resolutions. This result may suggest the idea that the selection of a particular approach by a social scientist is, to a great extent, predetermined by unconscious factors that can be disclosed by psychological analysis of his personality and sociological analysis of his milieu. The philosophical arguments that he offers for his point of view are taken to be only a 'rationalization,' an intellectual superstructure (erected *bona fide* in many cases) that functions as a sort of camouflage for the dominating non-theoretical motives of research and the social factors influencing them.

Investigations concerning the impact of the social milieu of research on its methods and results have attracted in the last two decades an ever-increasing number of sociologists and philosophers. The name 'sociology of knowledge' was given to this field of inquiry. The philosopher Max Scheler<sup>1</sup> wrote the first systematic treatise on this subject, but it is primarily the work of the sociologist Karl Mannheim that has made the sociology of knowledge what it is today. We are not concerned here with his remarkable achievements in this field but only with his view regarding the relations between the sociology of knowledge and epistemology. We shall first consider the history of the sociology of knowledge.

It may be traced back to the humanists and the philosophers of the Enlightenment who contended that the priests were eager to create and perpetuate superstition and prejudice among the people, but could not vanquish the irresistible tendency in human reason to free itself from these errors. Progress is marked by the milestones on the way toward this emancipation. In the first decades of the nineteenth century, historical processes were more thoroughly analyzed than in the preceding centuries, and, as a consequence, the idea of progress prevailing until then had to be revised. Hegel's philosophy of history and Comte's positivism with its law of the three stages are the most significant attempts in this direction. In his criticism of Hegel, Marx stated that the social conditions of men determine their thoughts and analyzed the origin of ideologies. This analysis was perhaps the strongest single influence on Scheler and Mannheim in their foundation of the sociology of knowledge.

Another important trend of thought pointed in the same direction. Darwin's theory of natural selection led to a biological interpretation of the idea of progress that was in some respects very different from, and even incompatible with, the traditional conception of progress as the continuous spiritual development of mankind.<sup>2</sup> For it suggested the idea that knowledge too, and in particular 'objective,' i.e. unprejudiced, appraisal of social facts is worth while only in so far as it serves as a weapon in the struggle for existence. But knowledge is not always such a weapon; often it has even an opposite effect in that it acts as a check on the vital energies. In such a case the quest for truth is 'unnatural.' This is taken to mean first, that such a quest is foredoomed to failure because it is contrary to the laws of nature, and secondly, that the goal is not worth striving for since it conflicts with the highest goals, those of life itself. This turn of thought also suggests a radical change in attitude toward the phenomena of the unconscious, in the investigation of which great strides were made at the end of the nineteenth century. Whereas for rationalism the unconscious always represented a stage of imperfection, it is now valued as a sort of bastion of vital forces,

guarding them against the incursion of 'disintegrative' reflective thought. Nietzsche, Pareto, and Sorel hold this view of the unconscious.

It has led to the thesis that errors in line with 'vital' interests cannot be overcome, since the forces of unconscious habits of thought are enlisted in their support. But the vital interests of different groups of men (particularly of the social classes of capitalists and workers) conflict with one another in the struggle for existence, and consequently consensus among them about what is true or false is regarded as unattainable in many instances.

At this point the question is bound to arise: Is it consistent to maintain the idea of objective truth at all when we have to admit that intersubjective agreement on truth is unattainable?

Marx had answered in the affirmative. According to him there are objective laws of history, and the proletariat, by virtue of its unique position in the historical process, is capable of grasping them. As a class it is immune to ideologies. Mannheim, on the contrary, answers negatively (with certain restrictions). He 'radicalizes' the ideology concept.

In what follows we shall set forth the chief theses of his doctrine in so far as they bear upon the problem of the objectivity of sociological thought.<sup>3</sup> ' . . . the process of knowing does not actually develop historically in accordance with immanent laws . . . [It] is influenced in many decisive points by extra-theoretical factors of the most diverse sort.'<sup>4</sup> The social process has a strong influence on the process of thinking. 'Underlying even the profound insight of the genius are the collective historical experiences of a group which the individual takes for granted, but which should under no conditions be hypostatized as "group mind."'<sup>5</sup> It penetrates even into the 'perspective of thought,' that is, the manner in which one views an object, what one perceives in it and how one construes it in thinking. Accordingly, the same word (e.g. freedom) may mean very different things when used by differently situated persons. Thus we come to a 'Revision of the Thesis that the Genesis of a Proposition is under all Circumstances Irrelevant to its Truth.'<sup>6</sup> One discovers the

relation between criteria of truth and the social-historical situation.

The abrupt and absolute dualism between 'validity' and 'existence'—between 'meaning' and 'existence'—between 'essence' and 'fact' is, as has often been pointed out, one of the axioms of the 'idealistic' epistemology and noology prevailing to-day. It is regarded as impregnable and is the most immediate obstacle to the unbiased utilization of the findings of the sociology of knowledge . . .<sup>7</sup>

[Epistemology] claims to be the basis of all science but in fact it is determined by the condition of science at any given time . . . the very principles, in the light of which knowledge is to be criticized are themselves found to be socially and historically conditioned. Hence their application appears to be limited to given historical periods and the particular types of knowledge then prevalent . . .<sup>8</sup> The theory of knowledge takes over from the concrete conditions of knowledge of a period (and thereby of a society) not merely its ideal of what factual knowledge should be, but also the utopian conception of truth in general, as, for instance, in the form of an utopian construction of a sphere of 'truth as such' . . . The particularity of the theory of knowledge holding sway today is now clearly demonstrable by the fact that the natural sciences have been selected as the ideal to which all knowledge should aspire. It is only because natural science, especially in its quantifiable phases, is largely detachable from the historical-social perspective of the investigator that all attempts to attain a type of knowledge aiming at the comprehension of quality are considered as methods of inferior value.<sup>9</sup>

In the mathematical method predominant in the abstract natural sciences we have 'truth as such,' which can be discovered by pure contemplation. An ideal of knowledge that has mathematics as its prototype is, however, inapplicable to social science.

Accordingly, the sociology of knowledge leads us toward a revision of the traditional epistemology with its ideal of 'truth as such.'

There are two different directions that may be pursued and it is still too early to say which one will be the more significant.

'One of the two directions taken by epistemology emphasizes the prevalence of situational determination, maintaining that in the course of progress of social knowledge this element is ineradicable, and that, therefore, even one's own point of view may always be expected to be peculiar to one's position.'<sup>10</sup>

This view may be called relationalistic, since it 'states that every assertion can only be relationally formulated. It becomes relativism only when it is linked with the older static ideal of eternal, unperspectivistic truths independent of the subjective experience of the observer, and when it is judged by this alien ideal of absolute truth.'<sup>11</sup>

In following the second way one 'will not absolutize the concept of "situational determination." As soon as I identify a view which sets itself up as absolute, as representing merely a given angle of vision, I neutralize its partial nature in a certain sense.'<sup>12</sup> As a consequence the task is set of synthesizing the various partial aspects.

The problem is not how we might arrive at a non-perspective picture but how, by juxtaposing the various points of view, each perspective may be recognized as such and thereby a new level of objectivity attained . . .<sup>13</sup> An effort must be made to find a formula for translating the results of one into those of the other and to discover a common denominator for the varying perspectivistic insights. Once such a common denominator has been found, it is possible to separate the necessary differences of the two views from the arbitrarily conceived and mistaken elements, which here too should be considered as errors.<sup>14</sup>

Only a social group that is not deeply rooted in tradition (*freischwebende Intelligenz*) will be able to perform such a synthesis, which will involve a formalization of social science. Important contributions have already been made, particularly by Simmel and Weber, to a theory of social forms, operating with such concepts as sub- and super-ordination force, obedience, subjectibility.

We shall now examine these arguments.

(a) *The influence of the social process on scientific thinking.*

This influence can hardly be denied, and in disclosing it the sociology of knowledge has made an important contribution to social science. It falls within the scope of such an investigation to determine the relative impact of theoretical and extra-theoretical factors on scientific progress. But in dealing with this problem, we must not presuppose that the extra-theoretical factors—social situations and trends—are devoid of theoretical elements. If we consider that anticipations of events in the social world are involved in social situations or trends, we shall realize that they cannot be adequately described without reference to theoretical factors, i.e. to knowledge, particularly sociological knowledge.

The discussion (as we find it in the work of Scheler and of Mannheim) concerning the relative influence of theoretical and extra-theoretical factors on the progress of knowledge is a reformulation of the problem how far man's thinking is determined by his social conditions. Since Marx 'put Hegel on his feet' this controversy has not ceased—which is not surprising if we consider the fundamental issue of social policy involved. If social existence, described in terms of economic (material) conditions, is taken to be the predominant factor in the history of mankind, and spiritual improvement a corollary of material improvement, then it seems proper to concentrate upon material improvement even if spiritual improvement is regarded as the ultimate end. Moreover, the issue seems to be of fundamental significance for social inquiry. If the material factors are decisive, then it would seem to follow that they should be regarded as independent variables in research and that all the other social phenomena should be explained in terms of them. This is indeed the guiding principle of Marx's materialistic conception of history.

In the next chapter we shall make some further remarks on this point, but in the discussion of the objectivity of social science we are chiefly concerned with the fallacy that has obscured this priority controversy as well as others—the fallacy of con-

founding relations of meanings with matters of fact, logical explanations with causal explanations.

Logically prior to the question of the origin of knowledge (the causal conditions of scientific progress) is the question of the meaning of 'knowledge,' in other words, of the distinction between warranted and unwarranted beliefs, and this distinction is *logically* dependent upon (defined exclusively in terms of) theoretical factors. 'Scientific truth' ('empirical validity') is defined in terms of rules of procedure that determine the grounds for the acceptance and for the elimination of propositions, and these grounds are propositions accepted at the time of the decision, i.e. theoretical factors. No extra-theoretical factors are criteria of truth. But to state this is not to deny that the acquisition of knowledge—in other words, scientific progress—is *causally* dependent upon extra-theoretical factors. What we reject is the thesis that this fact has any bearing upon methodological (epistemological) problems.

The issue of the relative significance of theoretical and extra-theoretical factors for scientific progress cannot be properly formulated without explicit reference to the rules of scientific procedure. The problem is one of causal imputation and, accordingly, is related to the problem of warranted predictions. It may be formulated as follows. Let K be the knowledge established in a particular science at a particular time  $t$ , and P a problem unsolved at time  $t$ : to predict how long it will take to solve the problem. The social situation (or trend) at time  $t$  will then be called relevant (or significant) for the scientific progress represented by the solution of problem P if, and only if, reference to this situation is required for warranting such a prediction.

In determining criteria for different degrees of significance, we shall have to consider what we gain in precision by adding data concerning the social situation. If no warranted prediction concerning the time required for the solution of the given problem can be made, it may still be possible to establish probability preferences (cf. Chapter VII) for such predictions, and with respect to these the meaning of 'relevance of extra-theoretical



factors' or 'degree of relevance' can be defined in a similar way.

Strictly speaking, there is always implicit reference to the social situation in predictions of scientific progress. It is presupposed that the social situation will not be such as to render 'normal' scientific research in general or research in a particular field practically impossible. But those who emphasize the significance of social situations or trends for scientific progress do not have this in mind.

An appraisal of the role of the scientific genius is important in this context if we suppose, as we generally do, that the emergence of a scientific genius is almost completely independent of the social situation. This problem too requires careful formulation, by which the type of achievement considered as peculiar to a scientific genius is made explicit. In the first place, we must make clear what we mean by 'originality of scientific achievement.' Broadly speaking, originality of achievement means independence of previously established achievements. It is clear, however, that no scientific achievement can be entirely original since it must be based on established experience, pre-scientific or scientific.<sup>15</sup> In judging the degree of originality of a scientific achievement, we have to appraise its difficulty, which implies an estimate of the relative scarcity of persons presumably capable of such an achievement.

In many instances it is possible to divide an extraordinary achievement considered as a manifestation of genius into a set of achievements none of which would be considered to require genius. It will then be appropriate to say that no genius was required for the scientific progress represented by the composite achievement, though its realization was accelerated by a genius. But there are extraordinary scientific achievements that are not divisible in this way—where the distance between two stages of scientific development had to be overcome by a mighty leap. Mannheim seems to hold that most of the achievements of the great social scientists are divisible in this sense, and I am inclined to share his view, but it is in need of support by further historical and psychological studies. Careful reformulation of

the problems of sociological imputation is suggested by this analysis.

(b) *The relation between epistemology and the sociology of knowledge.*

Mannheim is right in rejecting the claim made by some philosophers that pure reason can prove on ultimate grounds the exclusive adequacy of a particular method of empirical inquiry for the treatment of a given subject. Moreover he is right in rejecting those epistemologies which hold that passive experiences are the basis of all knowledge. We also agree with his thesis that natural science has been frequently regarded as the prototype for social science, which has led to an overemphasis on quantification. All these points have been discussed in previous chapters and need no further comment here.

But we cannot accept his statement that the idea of intersubjective validity is peculiar to the naturalistic view. This statement is in accord with Mannheim's general conception of the relation between the sociology of knowledge and the theory of knowledge, which we have now to examine. He is right in maintaining that 'empirical validity' is a relational term, but he is wrong in maintaining that it is logically related to the social setting of the scientist.

In raising this objection, we are not defending the epistemological view attacked by Mannheim. We agree with him in opposing the 'abrupt and absolute dualism' between existence and validity, as it may be found in some idealistic doctrines. The idea of two different, strictly separated realms of being, the realm of real objects and the realm of ideal objects, is untenable in the form in which it is usually presented; but in one respect there is indeed a fundamental difference between existence and validity. The fact that a belief of a particular kind is held is not a criterion of its correctness. The sociology of knowledge is concerned with conditions for the existence of beliefs; methodology (which supplants the kind of epistemology criticized by Mannheim once we have emancipated ourselves from the idea of absolutely cer-

tain knowledge of fact) is concerned with the criteria of their correctness, i.e. with the validity of the propositions under consideration.

The issue is complicated by the fact that one may ask for a causal explanation not only of the occurrence of particular beliefs, but also of the adoption of particular criteria for discriminating between warranted and unwarranted beliefs. The point may be made—and Mannheim (as well as Scheler) appears to make it—that differently situated social scientists adopt different rules of empirical procedure, and that relations can be established between variations in the social setting of social scientists and the rules of procedure they adopt. In order to determine whether such differences in the criteria of knowledge actually exist between two social groups and how far they extend, the rules of procedure implicitly acknowledged by each of the two groups would have to be made explicit. If fundamental differences were disclosed thereby and found to persist even after rectification of the rules (see Chapter III), it would be established that each group means something different by 'knowledge' and 'empirical validity of propositions.' An attempt could then be made to offer a sociological explanation of this fact. But this is not to say that the sociological facts referred to in such a definition 'enter into' the definition of 'knowledge' or 'empirical validity.' These terms are relative to rules of procedure but not to the causes for the adoption of particular rules. However, I do not think that such an inquiry would reveal fundamental differences in the rules of procedure adopted by different social groups.

An appearance to the contrary is created by the use of equivocal terms, which makes it seem as if two propositions that are indeed compatible contradict each other. Such equivocations, as we have pointed out in Chapter II, are frequently due to differences in the direction of research, in the selection of problems treated. No basic rules of procedure are involved here, though there may be disagreement concerning preference rules if the comparative significance of two problems in terms of broader theoretical goals is at issue.

The compatibility of different methods is often concealed by elliptical formulation of the pertinent problems, e.g. those of causal explanation. A problem of this kind is usually formulated as that of finding 'the' cause of an event, and explanations in terms of different causes are consequently taken to be exclusive of one another. That they are not is readily seen when we complete the elliptical formulations of the problems. The one-sided view that regards a particular causal explanation as exclusive of all others can often be traced back to the specific interests and attitudes of the social scientist who holds it. He who suggests improving the health of the people by slum clearance will be inclined to say that present housing conditions are 'the' cause of the present unsatisfactory state of health. Another, who proposes changes in diet as a means for the improvement of health, will declare that undernourishment is 'the' cause of poor health. Both are right in assuming that health would be improved by the acceptance of their proposals; either of them would be wrong if he maintained that health can be improved *only* by accepting his proposal. Errors of this kind can often be explained sociologically by reference to the existential situation of the scientist, but they do not cease thereby to be errors, and social scientists have the obligation of overcoming them. Mannheim is certainly right in emphasizing that this will be easier for men who are less deeply rooted in tradition, but whether easy or not, they have to try, and their scientific results will be judged in the light of their success. There cannot be differences irreconcilable in principle between the views of social scientists who agree about the rules of procedure of empirical inquiry. Disagreement concerning the rules, on the other hand, is disagreement with respect to the meaning of 'scientific knowledge.'

Mannheim seems to have been led to, or at least strengthened in, his opposite view by carrying too far the analogy between the 'position' and 'perspective' of a social scientist and the position and perspective of an observer of physical things. Let us examine this analogy. A man looking at a physical object from a certain place may either describe the object or report about his

visual experience. If the observer ascribes to the physical object all the properties of the content of his visual experience, he will be in error, and his error can be explained by reference to his perspective. But the error is not removed by declaring that his perspective 'enters into' his statement. If, on the other hand, he describes his visual experience of the object, the perspective is implicit in his assertion; but then he does not (strictly speaking) make a statement about the object, though some properties of the object may be inferred from this protocol proposition.

Applying these remarks to the different 'aspects' that social scientists in different 'positions' have of social objects, we see that these 'aspects' can be called 'perspectives' only in the first sense. We are concerned with the statements of social scientists about these objects, not with statements about their views of these objects. No data concerning the social scientists' 'positions' enter into their statements. The scientific situation to which the control refers may, of course, differ for social scientists in different 'positions,' and a statement warranted on the basis of one scientific situation may be unwarranted on the basis of another. But it is only the scientific situation—the body of established knowledge—not the scientist's social situation in general that enters into (is referred to in) the control of propositions of social science. If reference to the scientific situation is omitted in the (elliptical) formulation of a problem of control, then it seems as if there were different incompatible solutions of a single problem, whereas actually different problems are at issue. No question of synthesis arises here. If we were to speak at all of a 'general denominator' in such a case, we should have to assign this name to the generally accepted rules of procedure.

Another question is how the social scientist can free himself from the limitations of his perspective, i.e. how he can properly utilize all pertinent knowledge available in his time.

A social scientist who keeps his mind open to the arguments of his fellow scientists will more easily overcome the limitations of his perspective. A systematic clarification of the rules of procedure implicitly acknowledged by the community of scientists

will considerably facilitate this task by helping him in the appraisal of these arguments. It is to the lasting credit of Mannheim that he has thoroughly investigated the obstacles to mutual understanding among social scientists and that he has offered substantial aid in overcoming them.

No other social science has been more deeply concerned with the question whether it can attain and should strive for objectivity than has historical science. Ranke, one of the greatest and most influential historians of the nineteenth century, stated that it is the object of historical science to tell 'what it was that actually took place,' but this statement does not aid us in understanding the selective process in historiography whereby relevant and irrelevant factors are discriminated. A historian of the Civil War may declare that the letters written by Lincoln to his generals are historically relevant whereas some of the private letters he wrote during this period are not. Such differences are often simply taken for granted, and it might then appear at first as if relevance or irrelevance were intrinsic properties of events. But in fact they are relations, and statements concerning the relevance or irrelevance of historical facts that suppress this relativity are elliptical. In the first place, 'relevant' always means 'relevant for' something. For instance, Lincoln's letters to his generals were relevant for their strategical decisions and thereby for the outcome of Civil War battles. This type of relevance is 'causal relevance.' One means to say, 'Lincoln's letters have influenced the strategical decisions of his generals. If they had not received these letters, their strategical decisions would, in all likelihood, have been different.' This is a causal relation, and since causal relations are in terms of laws, explicit reference to the laws is also required in a complete formulation.

The issues of causal imputation that are implicit in the process of selection performed by the historian give rise to the methodological controversy whether a historical event is a *product* of the activity of the historian.<sup>16</sup> The relativists, who declare that it is, are prone to support their thesis by the general argument that causal relations are not 'given' but established by the investi-

ator. Nothing can be found in the events themselves—they argue—that would constitute a necessary relation among them. The particular type of correlation performed by the historian depends upon his conscious or subconscious interests, which are conditioned to a high degree by extra-theoretical factors. Their opponents reply by emphasizing that there must be intersubjectively discernible elements in the facts, which are the basis for their correlation. Otherwise such correlations would be quite arbitrary.

It can easily be seen from the preceding analysis what is right and what is wrong in these arguments. The relativists are right in insisting on the contingent character of causal relations, in rejecting the idea of a unique efficient cause of given events, and in emphasizing that the motives of a historian, which determine his selection of particular causal relations among events, can in many cases be traced back to the spiritual climate of his age, to his social setting, or to his personal equation. But this is not to say that the criteria of the validity of his statements concerning causal relations among historical events are determined by these extra-theoretical factors. Historical relativism (historicism), which claims that they are, disregards the implicit laws in terms of which 'historical causes' are established. A clear apprehension of the laws in terms of which an explanation is made is required of the historian no less than of the natural scientist, but it is more difficult for the former to meet this requirement. Not only are the laws in terms of which 'historical causes' are established more complicated than physical laws, but they are, moreover, only seldom *explicitly* given. This point has not received the attention it deserves, and as a consequence the task of making explicit the general assumptions implicit in historical imputation has not been squarely faced. If two historians, having at hand the same source material about the Civil War (which may be supposed to be the best available), disagree about the causal significance of Lincoln's letters to his generals on their strategy, we need not let the matter rest by declaring that the two conflicting views are equally supported by the facts. Facts (singular

propositions) alone do not support any causal imputation; they support it only if interpreted in terms of presupposed empirical or theoretical laws. While theoretical laws are not falsifiable in the strict sense, they are by no means exempt from control. Statements that experience must decide whether one can accept and uphold such laws, or that their application depends upon their fruitfulness (their success as working hypotheses) point to the fact that their control is intended, and indicate the task of making explicit the type of control implicitly referred to. Given the laws, we can distinguish between warranted and unwarranted historical imputations in terms of them. Whether one chooses to use the term 'cause' in this context is of little moment, but I should think that it may well be used. Objections to the use of the term are usually associated with the conception of causality as a necessary relation among facts that (supposedly) holds in the physical but not in the social world. We have seen that this view is untenable.

The issue between objectivism and subjectivism (relativism) loses its point at this level of clarity. Historical facts cannot speak for themselves as far as causal relations among them are concerned; they require an interpretation. This seems to involve subjectivity, but the interpretation has to be in conformity with presupposed theoretical laws, and these laws do not contain any reference to the historian and his perspective.

One formulation sometimes given to the subjectivistic view is that the historian 'distorts' or 'transforms' past facts. If this thesis is supposed to have any methodological significance, it cannot be interpreted as asserting that statements made by the historian are not in accordance with 'facts in themselves.' Rather, it has to be interpreted as asserting that the historian's statements are not in accordance with other statements, relating to the same topic, that are considered empirically valid. The rejected interpretation is a residue from an earlier, less sophisticated stage of the struggle against ideologies, when the historian was blamed for distorting the truth in order to serve vested interests. To what extent this was actually the case does not concern



us here, since it is not an issue of the methodology of historical science.

If, however, a modern writer declares that historiography distorts the facts, he usually does not mean to accuse historians of making intentionally false statements, but rather declares that the selection of data and the specific imputations characteristic of historiography are one-sided and fail to do full justice to the facts and the personalities described. The one-sidedness is regarded as either inevitable or capable of amendment by a synoptic view, the nature of which, however, is not clearly indicated. Sometimes it is claimed that the synoptic view is peculiar to a faculty of intuitive understanding. But this leaves unanswered the crucial question how the results of genuine intuitions are to be discriminated from those of pseudo-intuitions, in other words, how such results are to be controlled.

On the other hand, those who regard one-sidedness as inevitable are prone to support their thesis by reference to the finitude of the human mind. This conception, though fundamentally 'realistic,' is not very different from the 'idealistic' view that historical facts, *qua* historical, are the product of the historian's mind or—in the pragmatistic version—of the historian's activity, that the aspect or approach constitutes the historical object. On either view, the possibility of removing certain discrepancies among the historical interpretations of different social scientists is foreclosed.

Discrepancy, however, need not be incompatibility. Different aspects or approaches are, from the methodologist's point of view, different problems with certain common elements. In saying, for instance, that the historical Julius Caesar varies for different historical periods (apart from changes in the available source material), one implicitly refers to the different problems of determining Caesar's significance for different ages. One may discriminate between more and less significant problems in terms of given preference rules, but it would be inappropriate to say that a less significant approach distorts or transforms the facts.

Only exaggerated claims concerning actual or potential results of such an approach should be called distortions.

The relation of facts to presupposed values is considered essential in the process of selection performed by the historian. We shall briefly refer to this point in the following chapter.

No methodological controversies in social science have been more embittered than those relating to values. Time and again, particularly in the discussion of the basic issue whether the social sciences ought to be value-free, substantial arguments have been supplanted by arguments *ad hominem*, and the conscious or unconscious emotional motives 'behind' the different views have been examined rather than the intrinsic consistency of these views and their conformity with the presupposed rules of procedure. Those demanding value-free sciences reproach their opponents with placing obstacles in the way of a resolute search for truth for fear of possible consequences of unbiased inquiry for cherished prejudices and for vested group interests. The reply to this charge is either that social science cannot be value-free and that a particular kind of value judgment is smuggled into it under the cloak of the demand that it should be value-free, or that such a demand is a symptom of the moral callousness of those raising it. This kind of approach to methodological problems is apt to block the path to their solution.

The following discussion of this issue, as well as of other value problems in social science that are more or less closely related to it, will be based upon the results of Chapter ix. The crucial question is whether social science contains or ought to contain value concepts; and in dealing with it we must, in the first place, distinguish between value judgments and statements about emotional acts like loving, hating, desiring, fearing. The

study of such emotions, of their relations to particular types of persons, institutions, ways of living, and of their impact on social actions is, to be sure, of paramount importance for social science. But this is not the issue here; the issue is rather whether terms like 'good,' 'bad,' 'just,' 'unjust,' have a place in social science. The formulation of value judgments, we have pointed out, is usually elliptical; there is no explicit reference to the implicitly presupposed axiological rules. The complete formulation of a value judgment reveals that it is an analytic proposition. An appearance to the contrary is created by failure to distinguish between the value judgment proper and the statement that the object under consideration possesses the properties *by virtue of which* a value is to be assigned in conformity with given axiological rules.

For example, a particular action *a* is called 'good' in terms of the axiological rules of utilitarian ethics by virtue of its being conducive to the happiness of a person. The proposition, 'The action *a* is conducive to the happiness of a particular person' is synthetic; the proposition, '*a* is good in terms of given axiological rules by virtue of its being conducive to the happiness of a particular person' is analytic.

Being analytic propositions, value judgments cannot be part of the corpus of an empirical science, which consists exclusively of synthetic propositions. But this conclusion must not be regarded as a complete solution of the issue with which we are confronted. The decisive question is whether synthetic propositions containing value terms are admissible in social science. In order to answer it, we must remember what has been said in Chapter ix regarding the ambiguity of value terms arising from lack of consensus about the axiological rules determining their meaning. *Ambiguous terms should not, of course, be used in a science. However, if the ambiguity is removed by explicit reference to the presupposed system of axiological rules, there is no longer any reason to bar the use of value terms.*

The conclusions to be drawn from these observations for discussions of value problems in social science have already been

adumbrated in Chapter IX, but it will be well to deal with them in greater detail. If there is disagreement between two social scientists about whether a specific value is to be assigned to a particular person, action, institution, etc., the implicit statement of fact will first have to be separated from the value judgment in the strict sense. Differences of opinion about facts have to be settled in the ordinary way by checking the evidence (including laws) offered in support of each of the conflicting views. There is likely to be consensus about the way of checking the evidence, i.e. about the rules of scientific procedure. If differences are found, they must be precisely determined and, if possible, removed by methodological analysis.

If disagreement about value persists in spite of consensus about the facts, it may have its source either in a logical error committed in the judgment that the facts under consideration are (are not) subsumable under a given axiological definition, or in a difference of axiological rules. In the first case the disagreement is disposed of by correcting the logical error. In the second case the difference of axiological rules must be made explicit.

For example, if two social scientists A and B define 'just wage' in different ways, then A may declare a particular wage just, whereas B may declare it unjust, and both may make judgments that are correct in terms of the axiological rules each presupposes. Replacing the elliptical formulation of A's statement by the complete one, we may, for instance, obtain the statement 'The wage in question is just by virtue of the fact that it leaves the entrepreneur a profit of 5 per cent, in terms of a presupposed axiological rule that defines "just wage" in terms of the rate of profit left to the entrepreneur.'

On the other hand, the complete formulation of B's statement may read, 'The wage is unjust by virtue of the fact that it does not allow the worker a decent standard of living, in terms of an axiological rule defining "just wage" by the standard of living of the worker.' 'Decent' is again a value term that must be reduced to value-free terms by making explicit the properties by virtue of which a standard of living is called 'decent.'

Thus, the controversy between A and B seems to be resolvable into a 'verbal' difference. But there is a 'real' difference behind the 'verbal' difference between A and B. This difference is not fully characterized by pointing to the fact that either definition is indicative of the attitude of the social scientist who adopts it, that it is symptomatic of what he stands for. Either of the social scientists may be supposed to maintain that his is the 'true' definition which would have to be accepted by everyone who frees himself from prejudice. This assertion means that everyone reflecting thoroughly and without bias upon the fundamentals of his own pertinent valuations would have to recognize the definition as *adequate*. If this thesis is contested, the ensuing discussion may result in the modification of his definition if he recognizes that he did not take due account of all relevant factors. Such analyses are of great significance for social science, though they do not directly lead to changes in the corpus of a science.

Hence the postulate of value-free social science (ethical neutrality) must not be interpreted as implying that there is a specific kind of knowledge, namely value knowledge, to which, however, the social scientist is not permitted to refer. Rather, our postulate demands that the social scientist indicate the criteria of correct valuation which he implicitly presupposes when he makes use of value terms.

No social scientist has more thoroughly analyzed the issue of ethical neutrality than Max Weber. The following quotation expresses his chief point succinctly.

. . . there seems to me to be no doubt that, if maxims for actions possessing value are to be derived from valuations in the domain of politics [*praktisch-politische Wertungen*] . . . all that an *empirical* discipline can tell us in terms of its own resources is: (1) the necessary means, (2) the inevitable concomitant consequences, and (3) the resulting conflict of various *possible* valuations with respect to their various *practical* consequences. *Philosophical* disciplines, drawing upon their own resources, can, in addition, undertake to ascertain the 'meaning'

of valuations, thus revealing their ultimate 'meaningful' [*sinnhafte*] structure and their *meaningful* consequences, thereby determining their 'place' within the totality of possible 'ultimate' values and delimiting their meaningful spheres of application. But even such simple questions as: To what extent does an end justify the indispensable means? To what extent are we to resign ourselves to undesired consequences? And moreover, how are conflicts between various ends, whether they have their root in desire or in obligation, which collide *in concreto*, to be adjusted?—even these are entirely matters of choice or compromise. There is no (rational or empirical) scientific procedure of any sort that could yield a decision here. Least of all can our strictly empirical science pretend to spare the individual this choice, and therefore it should not create the illusion that it can.<sup>1</sup>

Let us examine this argument in the light of our analysis. The decisive point made by Weber is that the social scientist ought to be clear about the limitations of his knowledge and should not seek a rational or empirical justification of ends where none can be found. Ultimate goals are established by acts of will. Weber's formulation suggests that besides questions of value that are scientifically or philosophically decidable there are questions that are incapable of being answered theoretically but can only be resolved practically.

It should be clear from our analysis of value judgments that the value problems posed by Weber are incompletely formulated. If their elliptical formulation is replaced by the complete formulation, it will be realized that they are not insoluble. We shall then see that two different issues are involved, one presupposing the settlement of the other. The subordinate issue is that of a decision in terms of given axiological rules and established facts. The solution of such a problem is found by an analysis of meaning; it would be absurd to declare it insoluble. But behind the 'verbal' issue we find concealed the 'real' issue, whether there can be an ultimate justification for choosing one system of axiological rules rather than another. Ethical rationalism claims that this issue can be settled by pure rational

analysis of the meaning of value judgments. This claim is disputed by voluntaristic philosophers like Weber. We have already rejected (in Chapter IX) a radical rationalism in this respect, without, however, denying that rectification of axiological rules may carry us a long way toward removing differences.

Indeed, many problems concerning the adoption of axiological rules that have been regarded as insoluble can be solved in terms of generally acknowledged implicit axiological rules of second (or higher) order. For instance, in asking to what extent an end can be said to justify the indispensable means, we already refer implicitly to a system of axiological rules. The complete formulation of the question is: What actions, having a negative ethical value index under normal conditions in terms of a presupposed system of ethical rules, are to be assigned a positive value index if they are indispensable means to given positively valued ends? This question raises no other basic difficulties than the juristic question: What action, illegal under normal conditions, is prescribed if it is an indispensable means to ends that are valued positively in the frame of the legal order?

If one says that a person consults his conscience in order to reach a decision in a concrete case, one means that this person seeks to make clear to himself whether the action in question accords with his fundamental standards of valuation. The demand expressed in the *categorical imperative* that the result of the process of reflection should form the maxim of our action—in other words, Kant's definition of 'morally good will'—is accepted by Weber. He therefore attacks those academic teachers who wish to indoctrinate their students with certain political valuations, representing them as scientific or philosophical truths. Where this is done in good faith, that is, where the teacher is personally convinced that his political valuations can be proved scientifically or philosophically, an intellectual error may be involved. But such an error does not consist, as Weber thinks, in falsely assuming that problems can be solved which are in fact insoluble. *It consists rather in lack of clarity concerning the full meaning of the problem or, more precisely, of the two prob-*



*lems: (a) to prove that certain valuations are warranted in terms of given axiological rules by virtue of established facts; and (b) to justify these rules in terms of presupposed rules of higher order.*

We must distinguish between these problems and the problem how far the direction of social inquiry is or should be determined by non-theoretical goals. As far as the question of fact is concerned, it may be said that practical goals have often played a decisive role in the selection of problems. This holds even for historical science. One expects particular historical investigations to provide hints toward the solution of actual political or educational issues, and this expectation may determine the direction of inquiry to a notable degree. In social sciences that are not historical this relation is even more obvious, as a study of the development and the present state of the various branches of sociology and of political science reveals. But as soon as the process of inquiry indicated (though not uniquely determined) by the posing of the problem is initiated, the practical goals recede into the background. They determine the kind of knowledge to be sought after, but they are not directly relevant for the question how such knowledge is to be attained. We say 'not directly' because there is an indirect dependence. The choice of a particular method of inquiry for the solution of a given problem is determined in great measure by the state of knowledge in the field. However, this knowledge itself depends upon previous problems the origin of which may frequently be traced to practical goals. Moreover, every method not yet completely elaborated is connected with problems of its own, which are more or less independent of the primary problem. We may call them secondary problems. But in the actual process of inquiry primary and secondary (and perhaps tertiary, etc.) problems are inextricably interwoven. A problem may be posed for its own sake or for the sake of a practical interest connected with it or because of the significance it has for the treatment of other problems. The more a science expands and the broader its scope becomes, the more it strives towards autonomy in the

setting of its problems. New horizons of problems become visible as inquiry proceeds. Consideration of the present state of inquiry in the social sciences does not warrant the assumption that the majority of sociologists or economists will pose their problems with as little attention to practical demands as, say, astronomers do. Yet there is a noticeable tendency, particularly in economics, to let 'purely theoretical' considerations determine the path of inquiry without much concern about whether the results allow of immediate practical application.

Here the second problem mentioned above becomes acute. How close *ought* the contact to be between social science and practice? But in this form the problem is not univocally stated because the system of goals referred to is not indicated. The goals may be practical or theoretical. If the system is one of practical goals, the question is: How closely must science remain in contact with practice in order to serve it best? It need not be the case that the social sciences best serve the needs of practice when they restrict themselves to problems the solutions of which permit *immediate* practical application. If, on the other hand, the goals are theoretical, it is quite conceivable that closer contact with practice will be found conducive to their attainment. Such a point might be supported by the following argument: The opportunity for a thorough control of scientific statements is provided only where such contact is established, and this opportunity is indispensable for scientific progress; only where it exists does social inquiry receive that wealth of stimulation which it requires. Although, in my opinion, this argument must be somewhat qualified in order to be acceptable, it makes a point that deserves careful consideration.

But if, in dealing with this issue, the two related systems of ends are not kept sharply distinct, investigation of the pertinent problems is easily misled. It is then usually taken for granted that an inquiry that remains as close as possible to the actual problems of practice will do more for practice than one that ventures along paths leading a greater distance away from actual practical problems; and discussion usually centers about the

question whether the action of a scientist who is guided primarily by practical goals is morally better than the action of a scientist whose main motive is the desire for knowledge. In discussions of this sort, which are in some respects similar to those about *L'art pour l'art*, the untenable assumption is often made that the will to attain a practical goal which initiates an artistic or scientific activity remains the dominating motive in the course of the process. *Sacra auri fames* was the impelling motive for many of the greatest achievements of literature, music, painting, sculpture; but as soon as great poets, composers, or artists begin their work, they let themselves be guided by the 'inner law' of artistic creation. This holds equally for scientists, as can be attested by innumerable examples from the history of science.

Another issue is the logical relation between the meanings of 'theoretical goal of inquiry' and 'practical goal of inquiry.' By a practical goal of inquiry we understand a practical goal to be attained by the application of the results of inquiry, i.e. of solutions of scientific problems. Accordingly, the meaning of 'theoretical goal of inquiry' is logically implied in (logically prior to) the meaning of 'practical goal of inquiry.'

Not infrequently the question of the role of extra-theoretical goals in social inquiry is confused with the question of the role of the *analysis* of human goals in social inquiry. Closely connected with this question is that of the relation between causal and teleological methods and the importance of teleological concepts in the social sciences. There is no doubt that the study of human purposes, of the interrelations of ends, of their connection with appropriate means, generally speaking, of the whole means-end dynamics, assumes a central position in the social sciences.<sup>2</sup> In this respect, as we have pointed out in Chapters VIII and XI, the social sciences differ from the natural sciences, since psychological concepts have no place in the latter. But it is not advisable to restrict the use of the term 'cause' to relations among physical facts and thereby to overemphasize the contrast between teleological methods and causal methods in the strict sense. If, in agreement with pre-scientific and scientific usage,

'causality' is defined in the broader sense, two different types of causal relation are found to be implied in the teleological method, as we have shown in Chapter VIII.

Teleological concepts correspond to teleological methods, and there is no reason for disputing the legitimacy of such concepts in the social sciences. Caution is required, however, when one makes use of terms such as 'superindividual ends' or 'group ends' in sociological inquiries. In doing so, one must be clear whether and in what way these terms refer to ends in the strict sense, i.e. genuine psychical facts.<sup>3</sup> However, one must not mistake such problems of the analysis of meanings for problems of the explanation of facts. Thus, the question of the *nature* of group purposes, i.e. of the meaning of 'group purpose,' has time and again been confounded with that of the *origin* of group purposes.

The criticism of this fallacy frequently takes the form of asserting that there is a normative method relating to a sphere of values, of the 'ought,' and that the 'ought' is fundamentally different from the 'is.' It is maintained that the impossibility of deducing a norm from a statement of fact proves this dualism. But it follows from our analysis that neither this argument nor the thesis itself is tenable. They have their root in the elliptical formulation of norms that omits reference to the presupposed axiological rules. One thus fails to realize that norms are analytic propositions. The positive or negative value assigned to an action by a norm is not a peculiar property belonging to a separate ontological realm. The reason why the value of an action (the 'ought') cannot be deduced from the properties of the action (the 'is'), though it is assigned by virtue of these properties, is that the 'ought' is defined in terms of specific (axiological) rules. This is also the reason why value cannot be deduced from existence.

But to reject the preceding 'ultimate ontological justification' of a strict separation between causal and normative methods is not to deny that this separation is required. It is indeed of major significance in the methodology of the social sciences.

We shall illustrate this point by a brief reference to Hans

Kelsen's *Pure Theory of Law*, one of the most remarkable critical achievements in the social sciences. Kelsen's chief points can be summarized as follows: 'The definitions of legal concepts found in textbooks of legal science are for the most part sociological (genetic). The question of the 'nature' of law, of the state, of the legal person, of property, etc. is confounded with the question of their origin, and accordingly, the typical causal conditions under which, for example, a legal order or state comes into existence are referred to in the definitions of 'law' and 'state.' This holds particularly for the 'power theories' and 'consent theories' of law. The decisive argument of the pure theory of law against these definitions is that they are not in accord with the meaning the jurist actually assigns to the term 'law,' that they do not contain the criteria the jurist requires for discriminating what is law from what is not law. The law, as the jurist understands it, is not a state of affairs of a particular kind but a body of propositions of specific character (norms), the 'normative validity' of which cannot be derived from facts. The 'is' and the 'ought,' Kelsen says, 'lie in different planes.'

Analysis of the procedure of legal science is the only way toward an understanding of juristic concepts. There are two questions, namely: (1) What are the specific elements of meaning peculiar to legal sentences (*Rechtssatze*)? (2) How is it determined whether a legal sentence belongs to a given legal order? Two disciplines may thus be distinguished within the pure theory of law, namely, the *theory of legal sentences* and the *theory of the structure of the legal order*. According to Kelsen, all legal sentences are hypothetical propositions of the form: Under particular conditions compulsion ought to be exercised against particular persons. Now since the meaning of legal terms is correlative to that of legal sentences, it follows that all definitions of legal terms not containing the 'ought' are inadequate. The prevailing sociological (genetic) definitions of legal terms are exposed to this objection.

Kelsen has accomplished a 'purification' of juristic definitions on this basis, i.e. he has replaced definitions that do not cor-

respond to juristic usage by definitions that do. This was indeed a great achievement, but the philosophical reasons that he offered in support of his argument cannot be accepted. There is no 'normative sphere' that could be referred to in a normative method.

These remarks apply to his theory of the structure of the legal order. The pure theory of law has, to be sure, contributed much toward the understanding of the juristic method by clearly separating this question from that of the causes of the efficacy of norms, with which it had been confounded in the ambiguous phrase 'the binding force of legal norms.' It is not required, however, to resort to the assumption of a specific normative method in making this point.

That a given legal norm is valid within the frame of a given legal order *R* means that it falls under the definition of 'legal norm belonging to *R*.' We have already referred in Chapter III to the analogy between the structure of scientific procedure, where the validity of assertions is the issue, and the structure of legal procedure, where the validity of legal norms is the issue. There is, however, one important difference that we have now to emphasize.

'Validity of particular legal norms' is defined in terms of the validity of other legal norms. There is a chain of definitions that leads up to the constitution. But we cannot seek grounds for the validity of the constitution in the same way. The constitution is 'unconditionally valid' in terms of the implicitly presupposed definition of the particular legal order. Kelsen was led by an analogy with the idea of science as a hypothetico-deductive system to the interpretation that the assumption of the validity of the constitution is posited by a normative hypothesis, which he calls 'basic norm' (*Grundnorm*).<sup>5</sup> This basic norm he considers to be the ultimate ground of legal validity. But the basic norm is not a hypothesis at all; it is the definition of 'legal validity' with respect to a particular legal order.<sup>6</sup>

In summary we may say that reference to values or norms does not involve a specific method. This holds in particular for

'relation to values' (*Wertbeziehung*),<sup>7</sup> which Rickert regards as the selective principle in historical science. He makes the point that the historian, in the selection of historical material, is primarily guided by given values, that he sets as his goal the historical explanation of phenomena that are generally regarded as important for mankind, for example, political organization, religion, art. A historical fact in Rickert's sense is, accordingly a *value-related* fact. The historian has to establish axiological rules that determine by virtue of what properties objects are to receive value indices. Facts that are causally related to valued facts are selected as historically significant. But the value index as such does not affect the method.

Other value problems in social science are related to the concept of rational action. We shall discuss some of them in the following chapter.

THE attention of economists was early focused upon the problem of establishing a philosophical foundation for their science. When Adam Smith's ideas had been brought into systematic form by J. B. Say, and enlarged and refined by Ricardo, a theoretical system of thought was established unrivaled by any other social science. All the more important became the task of clarifying the foundations on which it rests. The prevailing view was that economic principles have their root in a fundamental property of human nature, in man's striving for the greatest amount of happiness attainable to him. Man, it was declared, 'is a being who is determined by the necessity of his nature to prefer a greater portion of wealth to a smaller one in all cases.'<sup>1</sup> It is usually admitted that this motive—the 'profit motive'—may be in conflict with other motives, especially with altruistic motives. But for the peculiar kind of phenomena with which economic analysis is primarily concerned, namely, market phenomena (in the broadest sense), the profit motive is of such predominance that it is appropriate to conduct the analysis of these phenomena on the presumption that human behavior in the market is governed by this motive alone. It is then investigated how a man would act in such transactions if he were promoting the goal of increasing his wealth as much as possible in the most appropriate way. This is the scope of economic theory.

In analyzing some of the chief methodological controversies concerning the meaning and significance of the principles of



economic theory, we shall attempt to show that most of these controversies involve the issues of general methodology discussed in Part I of this book.

In the first place, we must realize that the fundamental laws of the market are not empirical laws (synthetic universal propositions), since they are not considered as falsifiable by a single negative instance. The economic law 'If the price of a good is increased, the demand for this good will decline' may be taken as an example. Were this proposition to be regarded as an empirical law, it would have to be eliminated if one instance were found where a decline in the demand for a commodity did not occur after its price had been raised. And in fact many such instances have been found. But economists are not prepared to give up this law, though they may admit that its formulation is not adequate. Instead they propose such formulations as the following:

- (a) If the price of a good is increased, the demand for this good will tend to decline.
- (b) If the price of a good is increased, *ceteris paribus*, the demand for this good will decline.
- (c) If the price of a good is increased, the demand for it will decline under conditions of perfect competition.

However, the issue at stake is not disposed of by these more guarded formulations of the law. The restriction imposed by (a) and by (b) is meant as an exclusion of disturbing factors. Now we have emphasized in Chapter VI that this may mean two things. The disturbing factors may or may not be exhaustively enumerated. In the first case their exclusion amounts to the restriction of the generality of the law not containing such a clause. But if we now ask whether the economist would regard such a restricted law as falsifiable by a single negative instance, the answer will be again that he does not. He will argue that allowance must be made for exceptions, and this implies that he does not really consider it to be a synthetic proposition.

If, on the other hand, the kinds of disturbing factors are not exhaustively enumerated, it is always possible to say that the

non-conformity of the market situation with the law has its cause in some disturbing factor. Then, the law is not taken to exclude any possibility definitely, which means that it is not regarded as a synthetic proposition. Similar observations apply to (c). If the 'conditions' (more precisely, the criteria), of free competition are determined in such a way that they do not logically imply the law under consideration, then it is possible to interpret the law as a synthetic proposition. In such a case it would have to be regarded as falsifiable by one negative instance. But reflection upon how economists actually deal with it again reveals that they make allowance for exceptions, which excludes this interpretation. If, on the other hand, the 'law' is understood as a definition of 'free competition,' then we cannot make warranted predictions of market situations in terms of it.

Now we have pointed out in Chapters VI and XIV that the difficulty just mentioned disappears when we realize that predictions need not be deduced from synthetic universal propositions (in combination with singular propositions) in order to be considered as warranted. The underlying laws may be theoretical laws. This applies here. The law (c) states: Given an economy that possesses the properties in terms of which 'free competition' is defined. If it is established that the price of a good has been increased, it will be warranted to predict a decrease in the demand for it, unless it is also established that certain well-specified conditions (regarded as 'abnormal') prevail at the same time. If, on the other hand, the demand for a good decreases, and it can be shown that this decrease was preceded by an increase in the price of this good, the economist will, 'under normal conditions,' regard the latter fact as a satisfactory explanation of the former.

If a prediction in terms of a theoretical law is not fulfilled, we need not eliminate it for this reason. But, as we have pointed out, the status of the law is not unaffected by the success of such predictions. Theoretical laws are controlled by rules of higher order that refer to the success of predictions made in terms of these laws.

Similar considerations apply to the other fundamental laws of the market, such as the cost principle according to which the price of a good tends to be equal to the cost of its production. Something must be said in this context concerning the meaning of the term 'tendency.' In the statement that there is a tendency for the price of a good to be equal to the cost of its production, the term 'tendency' denotes an approximation to a given point. The term has no such meaning in the statement that the demand for a good tends to decline when its price increases. There is, however, one meaning of 'tendency' that applies in both cases and corresponds to the *ceteris paribus* clause.<sup>2</sup> This clause poses a problem or, what amounts to the same thing, it points to a certain direction of inquiry. If the relation between two events is not in accordance with a presupposed theoretical law, one does not allow the matter to rest there. One will ask for an explanation of this fact in terms of a law referring to changes in some factors not referred to in the theoretical law under consideration. If an increase in the price of a commodity is not succeeded by a decline in the demand for it, one will investigate whether other factors presumed to be relevant in such cases (e.g. the purchasing power of typical consumers or the prices of substitutes for the commodity) have remained unaltered. Should the examination of these factors not lead to the desired explanation, the economist will direct his research toward the determination of other factors that may be relevant in this context, i.e. he will attempt to establish new theoretical laws. In the attempts to explain deviations from the fundamental laws of the market, similar functions are assigned to already established disturbing factors and to factors not yet established. *This is the reason why they are not properly distinguished in the ceteris paribus clause.*

Failure to discriminate between them may, however, have serious consequences in the prediction of economic events. Let us suppose an economist makes predictions about changes in the demand for commodities solely on the basis of changes in their prices without taking into account other economic or non-economic data relevant to the prediction. When his prediction

is not fulfilled, he may impute this outcome to 'disturbing factors.' But the influence of such disturbing factors need not have been—though it may have been—unpredictable in the situation in which the prediction was made. If it was predictable, he can rightly be blamed for not having considered them.

Reference to the *ceteris paribus* clause was often made in support of exaggerated claims concerning the significance of the fundamental laws of the market for the prediction of economic events. Such claims led to a strong reaction, represented by the historical and institutionalist schools. It was in some respects a sound reaction. But the proponents of these doctrines went too far in their criticism, because they failed to realize that the errors they attacked are not inherent in economic theory itself, but only in its inadequate interpretation.

The method of isolating a small number of factors and of analyzing their interdependence is common to all empirical sciences that are not merely descriptive. Francis Bacon's advice to 'dissect nature' suggests such a method, and Galileo's and Newton's work is the most striking proof of the soundness of the advice. They were as vigorously attacked for being too abstract as the theoretical economists are in our own days. Galileo was blamed for confounding such different movements as the swimming of fish and the flying of birds, and Newton for transforming living nature into a corpse. But no one was better aware than Galileo that the method of isolation needed a method of synthesis as its complement. And it is just as proper for the theoretical economist as for the theoretical physicist to produce 'semi-finished goods.' But the former is more easily tempted than the latter to mistake them for finished goods and to sell them as such. This temptation has been increased by aprioristic arguments according to which the laws of the market can be derived from self-evident principles of human behavior. All these arguments suffer from the fallacy of confounding synthetic and analytic propositions, matters of fact and relations of meanings.

But the rejection of pseudo-philosophical 'ultimate' grounds for the validity of the laws of the market, which are supposedly

derivable from the nature of man, is not bound to lead us to the thesis that they have no other basis than actually observed behavior in the market. Analysis of human motives plays a significant part in their foundation.

To make this point clear, we shall now analyze the conception of rational behavior, which occupies a central position in economic theory. In the attempt to establish a philosophical foundation for economics, this concept has frequently been associated with the idea of the highest good in ethics. Efforts have been made from the physiocrats down to our own age to justify the profit motive in terms of classical moral philosophy or of utilitarianism, to prove that the goal of maximizing profits is a rational goal. But it is now almost generally recognized that the distinction between rational goals and irrational goals is of no significance for a methodological analysis of the principles of economic science. The goal of maximizing profits is presupposed, and 'rational economic behavior' is defined in terms of this goal.

However, because the other dimensions of relationality of the term 'rational behavior' are not clearly grasped, difficulties have emerged in the interpretation of economic theory. We have emphasized throughout this book that propositions concerning the correctness of human behavior prove to be analytic as soon as their elliptical formulations are complemented by explicit reference to the implicitly presupposed rules in terms of which 'correctness' is defined. The term 'rational behavior' may be replaced by 'correct behavior,' and, accordingly, the results of our analysis concerning correct behavior in general apply here. Given the alternative of buying a pound of cherries of the same kind at 12 cents at store A or at 14 cents at store B, the proposition 'It is rational to buy the cherries at store A' is an (elliptically formulated) analytic proposition if 'rational behavior in the market' is defined in terms of buying at the lowest possible price. We have intentionally chosen a trivial case for illustration, but nothing essential is altered if we replace it by a more complicated example of economic theory, e.g. by Ricardo's theory of

comparative costs in international trade or Cournot's theory of monopoly.

Let us consider the behavior of Cournot's<sup>3</sup> monopolist—the owner of a mineral spring. Cournot presupposes that the demand for his mineral water will vary with the price per bottle and that the demand curve and the cost curve (correlating the amount of output with the costs per unit of output) are completely determined. Now let  $p$  be the price per bottle,  $\varphi(p)$  the demand corresponding to this price, and  $\psi(p)$  the total costs of an output exactly covering this demand. Then the maximum profit attainable is seen to be the maximum of the function  $p \cdot \varphi(p) - \psi(p)$ , which can be found by the differential calculus. The behavior of our monopolist is then called 'rational' if he fixes his price and his level of output in such a way that the maximum profit results. This seems to be obvious. However, methodological difficulties arise. In order to determine the optimum monopoly price and the optimum level of output, the monopolist has to know (a) the cost function  $\psi(p)$ , (b) the demand function  $\varphi(p)$ , and (c) the maximum value of the function  $p \cdot \varphi(p) - \psi(p)$ . But knowledge of (a) and (b) is taken to require perfect foresight. On the other hand, the point has been made that the assumption of perfect foresight is not admissible in economic theory, since man cannot have such knowledge. It has even been shown that the assumption of perfect foresight may lead to inconsistencies within economic theory.<sup>4</sup> Nevertheless, this assumption was regarded as an essential part of the theory by most economists until about twenty years ago and is still so regarded by many.

There is little doubt that both the failure to distinguish clearly between rational and empirical knowledge and the view that the former is the prototype of the latter are largely responsible for this idea. The answer to the question 'What knowledge will enable the monopolist to determine the price of his commodity so as to yield him maximum profit?' accordingly seems to be: He must have both the mathematical knowledge that enables him to solve the problem of the maximum value of the function,

and perfect foresight in determining the demand curve and the cost curve. But the conception of perfect foresight is by no means clear. One might be tempted to declare that perfect foresight is given where a prediction is precisely fulfilled. However, even those who hold this view would probably hesitate to say that a person who successfully predicted what numbers would be drawn in a lottery had perfect foresight of the outcome. They would rather say: He had good luck in making his guess, but he could not know what numbers would be drawn, even though he might claim to have known and might defend his claim by invoking the authority of an Egyptian dreambook. For this prediction, though fulfilled, was not warranted (in terms of the presupposed rules of procedure) by the then existing scientific situation.

We have emphasized in Chapter VI the distinction between warranted and fulfilled (successful) predictions. A prediction may be warranted and successful, warranted but unsuccessful, unwarranted but successful, unwarranted and unsuccessful. If a number of predictions that were unwarranted in terms of given procedural rules prove to be successful, then it will sometimes be in conformity with presupposed rules of higher order to alter those rules in such a way that the successful predictions are warranted in terms of the new rules. But it should be noted that the conditions for the elimination of theoretical laws are highly complex. They are usually related to the whole theoretical framework of the science under consideration. Scientists would hardly be induced even by a rather large series of successful predictions of lottery numbers in terms of precepts in an Egyptian dreambook to accept these and to abandon their statistical theory. We are prone to overrate the scientific importance of the success of a prediction because of its practical importance. If a prediction in social science is fulfilled and the actions based on it lead to the desired result, we are usually satisfied and do not care to investigate whether the prediction was warranted. But the scientific issue involved is whether future predictions of this kind should be guided by the same rule, and this issue may also be of major practical significance. The saying 'The proof of the

pudding is in the eating' may be sound enough for those who eat a particular pudding and do not want to concern themselves with the intricacies of the art of cooking, but it does not provide a recipe for the cook.

We penetrate to the core of the issue of perfect foresight by applying the results of our analysis in Chapter IV of the meaning of 'truth of synthetic propositions.' The conception of perfect foresight relates to an idealized scientific procedure, to 'complete' knowledge of facts and laws. It is thus defined in terms of rules of scientific procedure. But it has no procedural significance.

The economist, or rather the interpreter of economic theory, places himself in the situation of the economic subject—in our example a monopolist—who is confronted with the problem: How shall I fix the price and level of output in order to obtain maximum profit? He may err in three respects, namely: (a) in his estimate of the demand function, (b) in his estimate of the cost function, and (c) in his calculation of the maximum profit in terms of these functions. In this sense, (c) is co-ordinated with (a) and (b). But this should not prevent us from realizing that (c) has a logical status different from that of (a) and (b). The propositions establishing the pertinent demand function and cost function are synthetic; the statement that, given the demand function and the cost function, a particular price in combination with a particular level of output yields maximum profit is an analytical proposition, in other words, it is deducible from the demand function and the cost function. While it thus presupposes the 'givenness' of these two functions, it does not presuppose the correctness of the procedure leading to their establishment. A prediction can be called 'rational' or 'correct' or 'warranted' in terms of the accepted rules of scientific procedure if it is inferred from the whole body of (relevant) knowledge available at the time at which it is made, which implies, for instance, that risks are estimated in terms of given statistical laws. By carefully distinguishing between 'logical rationality' and 'empirical rationality' and, accordingly, by replacing 'perfect foresight' by 'warranted prediction,' we arrive at a better understanding of the



empirical significance of economic theory. We realize that economic theory contains implicit presuppositions concerning the factors considered as relevant in making predictions about market phenomena.

The theoretical economist will not engage in a detailed examination of such technical, psychological, or institutional factors, but he will outline the chief kinds of factors that should be taken into account and will thus indicate a certain direction of inquiry. In what follows we shall illustrate this point by a brief analysis of the meaning of the marginal utility principle in economic theory.<sup>5</sup>

The problem leading to the formulation of this principle is: Given an economic subject who is perfectly clear about his goals and their relative rank and, moreover, about the functions that a unit of any of the different goods under consideration may have in the promotion of these goals: to determine how such an economic subject is to decide whether he should rather forego a unit  $g_1$  of a good  $G_1$  or a unit  $g_2$  of a good  $G_2$ . The answer offered to this question is: He will forego that unit on the possession of which less utility depends. This means: Given a stock of commodities  $c$  in the possession of the economic subject that includes the two units  $g_1$  and  $g_2$  and given a set of goals that can be attained if optimal use (in terms of the system of goals of the economic subject) is made of each unit of each commodity. Let us symbolize by  $S_c$  the totality of goals (satisfactions) attainable by the use of all elements of  $c$ . Those attainable by the use of all elements of  $c$  with the exception of  $g_1$  or  $g_2$  may be called  $S_{c-g_1}$  and  $S_{c-g_2}$  respectively. It is then rational (correct) to forego  $g_2$  rather than  $g_1$  if  $S_{c-g_2} > S_{c-g_1}$ . If the converse relation holds, it is rational to forego  $g_1$  rather than  $g_2$ . The utility depending, under these conditions, upon a unit of a given commodity is called the *marginal utility* of this unit. According to the principle of marginal utility a rationally acting economic subject will evaluate any unit of a given commodity according to its marginal utility. In other words, its status in the plan of the economic subject is determined by the least important use assigned to any

unit of this good within the frame of the plan. Let us consider, for the sake of illustration, an economic plan containing the following three items in order of importance:

1. Use of one sack of wheat for food.
2. Use of one sack of wheat for fodder.
3. Use of one sack of wheat for sowing.

Any sack of wheat being usable for any of the three purposes, the relinquishing of one sack will mean that the sowing will have to be given up; and the value of one sack will be assessed accordingly. It may thus be said that the principle of marginal utility can be derived from the concept of rational economic behavior, which means that 'rational economic behavior' is defined in terms of this principle.

But as soon as this is recognized, the problem arises what such a definition can contribute to the explanation of economic reality. The answer is that it suggests a broad program for economic research. This may be formulated as follows: In order to establish how a unit of a certain commodity will be valued by economic subjects, one should first determine their goals and the order of preference among them; and then, the possible uses of units of this commodity in the promotion of these goals. In doing so, one must consider what is called the technical complementarity of goods, i.e. the combinations of commodities required for their use in accordance with the given ends.

In the usual less general formulation of the principle of marginal utility, frequently called the principle of diminishing marginal utility, one presupposes—according to Gossen's first law—that beyond a certain point an additional unit of a consumer's good will yield less satisfaction than previous units have provided. One has therefore to find the points of saturation.

When the value of capital goods (goods of higher order) is to be determined, the values of consumers' goods are to be regarded as 'given,' i.e. as independent variables, and from them the values of capital goods are to be derived. This is the methodological core of the statement that the value of a capital good depends upon the marginal utility of its marginal product. The

methodological precept involved may be broadly characterized as follows: To determine the value of a capital good, one should start with a listing and ranking of those consumers' goods for the production of which the capital goods may be used. Then one should determine what loss in consumers' goods would result from the loss of one unit of the capital good under consideration. Attention should be given in this context to the technical complementarity and the substitutability of capital goods in particular productive combinations.

However, a major problem arises for proponents of the subjective theory of value. How can the principles of an exchange economy, in particular of a money economy, be derived from this theory?

The problem was posed in the form: How can prices be derived from economic values (or marginal utilities)? And thereby the stage was set for an intense and persistent methodological controversy.<sup>6</sup>

If the prices of goods—it was argued by one side—are to be derivable from their marginal utilities, as the subjective theory of value assumes, then prices must be 'contained in' marginal utilities; the order of marginal utilities (order of values) must be conceived as an order of *extensive* (measurable) magnitudes. This—it was replied by the other side—is impossible, for marginal utilities (values) are *intensive* magnitudes, and hence, by their very nature not susceptible to measurement; they cannot be transformed into extensive magnitudes.

The latter argument may seem convincing at first sight, but we should not too readily accept it. Those who make this point can be refuted by reference to the fact that physical methods provide for measurements of temperature, brightness, pitch. It would be strange indeed to reject these methods as 'illogical.' But in granting the measurability of marginal utilities we must not erroneously assume that the numerical values to be arrived at are contained as *qualitates occultae* in the phenomena to be measured. We have already referred to this point in Chapter III. With respect to temperature, we have pointed out that spatial

magnitudes (e.g. lengths of mercury columns) are correlated with temperature sensations, and inferences are drawn from the state of the thermometer to our sensations of warmth, and conversely. Whether one says that, in the process described, only the lengths of mercury columns are measured but not temperature, or whether one uses the term 'indirect measurement' is a question of secondary importance. But the introduction of this term is convenient.

Economists who dispute the measurability of marginal utilities (economic values) are consequently right in denying that utilities can be measured directly, but it would be wrong to deny that they can be measured indirectly. What can be demanded is that those who deal mathematically with marginal utilities indicate explicitly the theoretical laws (rules of correlation) that they implicitly presuppose. After this has been done, there may still be disagreement concerning the empirical significance of the proposed correlation—an issue to be decided in terms of procedural rules of higher order—but the original controversy has lost its point.

The issue has been obscured by failure to separate the problem of establishing the appropriateness of a method from the problem of clarifying the meaning of the method. One side in this controversy denies that marginal utilities can be measured. The other side retorts: 'We have measured them; *ab esse ad posse valet consequentia*.' But the apparently irreconcilable conflict between these views is removed as soon as the meaning of 'indirect measurement of marginal utilities' is clarified. It is then seen that actual methods of measurement cannot be rejected as logically impossible. They can be challenged only on empirical grounds, by contesting the validity of the laws in terms of which the correlation is made. The proponent of a method of measuring marginal utilities may be criticized if he does not interpret his own method adequately, but such a criticism does not imply an objection to the empirical significance of the proposed method. The social scientist must constantly be on his guard against confounding these two kinds of argument.

The tendency to do so is strengthened by the unrealizable desire to find ultimate grounds for the acceptance or rejection of methods of empirical science. As we have already mentioned in the introduction to this chapter, this tendency, noticeable in economic theory from its very beginning, was strengthened by its connection with Bentham's philosophy. In some of the basic works of the marginal utility school it is particularly apparent, but I have nowhere found a more precise and impressive presentation of the aprioristic thesis than in L. von Mises' writings. The following quotations will give a fair idea of his point of view. It should be noted that von Mises gives the term 'action' the meaning of 'rational action' and treats 'rational action' and 'economic action' as synonyms.

Human action is conscious behavior of people, which we can distinguish in a sharp and precise way from unconscious behavior, although it is perhaps in some cases not easy to determine whether given behavior is to be assigned to one or the other category . . .<sup>7</sup>

As thinking and acting persons, we grasp the concept of action. In grasping this concept, we simultaneously grasp the correlated concepts of path and goal, means and end, cause and effect, beginning and end, as well as the concepts of value, good, exchange, price, cost. They are all necessarily implied in the concept of action, and together with them the concepts of valuing, order of rank and importance, scarcity and abundance, advantage and disadvantage, success, profit and loss. The logical unfolding of all these concepts and categories in systematic derivation from the basic category of action and the disclosure of the necessary relations among them constitute the first task of our science. The part of the doctrine that deals with it is elementary value- and price-theory. There can be no doubt as to the aprioristic character of these disciplines . . .<sup>8</sup>

The most general condition of action is a state of dissatisfaction on the one hand and the possibility of removing or lessening it by action on the other . . . Only this most general condition is necessarily presupposed in the concept of action. The other categorial conditions of action are independent of the general

concept of action; they are not necessarily assumed as conditions of concrete action. Whether or not they are given in a particular case can be shown by experience only. But where they are given, the action necessarily falls under definite laws that flow from the categorial determinacy of these further conditions . . .<sup>9</sup>

Whether the exchange of economic goods (in the broadest sense of the concept, which also includes services) occurs directly or by the mediation of a means of exchange, can be established only empirically. However, where and in so far as means of exchange are employed, everything that is essential for indirect exchange must become actually operative. Everything asserted by the quantity theory, the theory of the relation of quantity of money and interest, the theory of currency and the circulation-credit theory of the business cycle, becomes inescapably connected with action. All these propositions would also be meaningful even if there had never been any indirect exchange; only their practical significance for our action and for the explanation of reality would then have to be appraised differently . . .<sup>10</sup>

All propositions that our universally valid theory can establish hold to the extent that, and in so far as, the conditions they presuppose and precisely delimit are given. Where these conditions are given, these propositions hold without exception. This means that these propositions concern action as such, i.e., presuppose only the fact of a dissatisfaction, on the one hand, and the recognized possibility, on the other, of relieving this dissatisfaction by conscious action, and that, therefore, the elementary laws of value are valid without exception for all human action. Where an isolated person acts, his action occurs in accordance with the laws of value. Where, moreover, goods of higher order are introduced into action, all the laws of the theory of imputation are valid. Where interpersonal exchange appears, all the laws of price are valid. Where indirect exchange occurs, all the laws of monetary theory are valid.<sup>11</sup>

It should be clear from the analysis throughout this book that we cannot accept von Mises' argument. To derive the concepts 'value,' 'good,' 'exchange,' etc. from the basic category 'action' and to establish necessary relations among these concepts is to operate with analytic propositions. It is impossible to arrive at

any synthetic proposition (whether categorical or hypothetical) about economic reality through these operations. Hence it is inappropriate on the one hand to claim *a priori* validity for the laws and on the other to declare that certain conditions are required for their validity. The logical analysis of concepts is not relative to any conditions.

There are two different ways of defining economic concepts. They may be defined in terms of economic laws, and then the latter 'hold necessarily for these concepts' but contain no assertions about reality. In this case an action cannot be called, say, 'exchange' unless it is in accordance with the 'laws of exchange.' If, however, economic concepts are not defined in this way, necessary validity cannot be claimed for any of the laws relating to the economic facts that they designate. Predictions in terms of these laws may or may not be fulfilled.

A number of similar ambiguities can be found in the interpretation of the subjective theory of value, e.g. in the thesis that an economic subject who has to forego the satisfaction of one of several needs will necessarily forego the satisfaction of the least important need. Such 'necessity' is given only if 'the least important need among several needs' is by definition 'the need left unsatisfied.'

But, one might ask, do not principles of economic theory have a *normative* validity independent of the actual behavior of men and is not this behavior evaluated as 'rational' only if it conforms with these standards? And does not such a judgment, arrived at by a normative method, have *a priori* validity? An answer to this question is clearly determined by the preceding analysis.

'Rational action' is defined in economic theory in terms of the maxim 'Strive for the greatest amount of profit.' The sentence 'A certain action is rational'—without indicating in terms of what rule it is rational—is elliptical. As soon as we complete it, we realize that there is no reference in economic theory to a peculiar normative validity or a normative method.<sup>12</sup>

Most critics of classical and neo-classical economic theory who have objected to its deductive method have opposed the

claim that indubitable statements about economic reality can be derived from *a priori* principles. But as we have mentioned, it has often been overlooked that these objections do not hold against economic theory properly understood but only against its inadequate interpretation. The theory itself will, no doubt, have to be modified—important modifications have indeed recently been suggested—but it is unlikely that it will be replaced by a radically different scientific approach to economic phenomena.



IN what follows we shall review some of the chief results of our analyses in the preceding chapters.

1. The most persistent controversies over methods in social science have their roots in issues of general methodology that concern the principles of scientific procedure.
2. By scientific procedure we decide whether given synthetic propositions should be considered parts of the corpus of a given science. Decisions to change the status of a proposition in this respect are called scientific decisions. Unambiguous meanings of the sentences concerned are presupposed in scientific decisions.
3. Such decisions must not be arbitrary; grounds must be given for the incorporation of a proposition into or its elimination from science (methodological principle of sufficient reason). But the term 'ground' is used with two different meanings. That a set of propositions  $p_m$  is the ground of a proposition  $p_n$  may be taken to mean either that  $p_n$  is deducible from  $p_m$ , or that  $p_n$  can be accepted on the basis of  $p_m$  in accordance with the rules of scientific procedure even though it is not deducible from  $p_m$ .
4. Thus, we must observe the fundamental distinction between deductive reasoning in the strict sense, which is exclusively concerned with the internal relations of propositional meanings, and inference in scientific procedure, such as inductive

inference, where the empirical validity of propositions is at issue. The rules of deductive reasoning (e.g. the valid moods of the syllogism) can be 'ultimately justified' by reference to propositional meanings. This does not hold for the rules of inductive inference, and, in general, for the rules of scientific procedure. There is no ultimate justification of these rules; we cannot go beyond them in discriminating between correct and incorrect scientific decisions.

5. Failure to distinguish between deductive logic in the strict sense and the logic of scientific procedure (methodology) leads to an inadequate (elliptical) formulation of scientific problems, methods, and solutions. Replacing the elliptical by complete formulations, we see that it is possible to prove the correctness of scientific decisions *in terms* of the rules *on the basis* of established knowledge. In other words, 'Correct scientific decision' is defined in terms of the rules. *The logic of science (methodology) is the theory of correct scientific decisions.*
6. We defined:
  - (a) 'step' as 'a correct scientific decision that is not the compound of two or more correct scientific decisions.'
  - (b) 'scientific situation' as 'the knowledge considered as established at the time of a scientific decision.'
  - (c) 'verification of a proposition  $p$ ' as 'correct acceptance of a proposition  $p$  into science.'
  - (d) 'invalidation of  $p$ ' as 'correct elimination of  $p$  from science.'
  - (e) 'falsification of  $p$ ' as 'invalidation of  $p$  combined with verification of a proposition logically incompatible with  $p$ .'
7. The rules of scientific procedure for different sciences are different in various respects, but there are certain fundamental properties of the system of rules that are regarded as essential for scientific procedure in general. Such principles are:

- (a) The grounds of a scientific decision must be among the propositions belonging to the scientific situation to which the decision is related.
- (b) Observational reports (protocol propositions) play a key-role among the grounds.
- (c) All scientific decisions are reversible (principle of permanent control).
- (d) A scientific decision must not lead to a scientific situation containing two incompatible propositions (procedural correlate of the principle of contradiction).
- (e) No proposition can be in principle undecidable (procedural correlate of the principle of the excluded middle).

Principle (e) is not generally accepted today.

- 8. The observational test is interrelated with other types of control of propositions and cannot be properly understood if we isolate it from the other rules of procedure. It has been isolated from them by the proponents of a correspondence theory of truth, who claim that the truth of propositions of a certain class (called 'basic propositions' by Russell) is definitely ascertained by an immediate experience and is thus beyond further control. Proponents of a coherence theory of truth have emphasized the interrelation of the various types of control. But they have failed to make it clear that 'coherence' must be defined in terms of rules of procedure.
- 9. An adequately formulated coherence theory of truth will not state that truth is created by and is thus dependent upon actual verification; it will rather state that the meaning of 'truth' is defined in terms of the rules of verification and invalidation. It is misleading to say that verification creates truth even if the terms 'true proposition' and 'verified proposition' are considered synonymous. Frequently, however, the term 'true' is not related to actual procedure but

to possible procedure, and truth is understood as an ideal. We then mean by a 'true' proposition one that could be accepted if we had all knowledge relevant for the scientific decision concerning its acceptance and that, once accepted, could withstand all possible controls.

10. The term 'theoretical goal of inquiry' is definable exclusively in terms of the rules of procedure. The distinction between question and possible answer on the one hand and (empirical) problem and solution on the other corresponds to that between deductive logic and the logic of scientific procedure. Whether a given indicative sentence is a possible answer to a given question (interrogative sentence) is determined exclusively by the meaning of the two sentences. No reference to procedural rules is involved. But in posing a problem, we set the goal of finding a *correct* answer to a given question, and this brings the procedural rules into play.
11. We have called 'preference rules of procedure' as contrasted with basic rules, those referring to theoretical goals. One class of preference rules relates to the presumable relevance of a particular type of procedure for the solution of given problems, another class to the degree of significance of given problems in terms of theoretical ideals, such as unity, simplicity, universality, and precision of laws. The basic rules of procedure, on the other hand, define 'correct scientific decision in a given situation,' without reference to the goals of inquiry.
12. It is not only the corpus of a science that is changed in the course of inquiry; the rules of scientific procedure too are subject to change, but in contrast to changes in the corpus of science, those in the rules are not unlimited. The fundamental properties of the system of rules are invariable. 'Correctness of decisions to change rules of procedure' is defined in terms of *rules of higher order*. In asking whether it is correct to incorporate a proposition or to eliminate a hitherto incorporated proposition, we presuppose procedural

rules of the first order; in asking whether a change in a procedural rule of the first order is correct, we presuppose rules of the second order, etc. It is therefore inconsistent to regard such questions as meaningful and at the same time to deny that the pertinent rules are 'given.' But in many instances they are not *clearly* grasped. It is then the task of the methodologist to make them explicit.

13. By 'laws' we understand universal propositions from which, in combination with singular propositions, predictions can be obtained. Analysis of the control of laws reveals that they may be either synthetic universal propositions or rules of procedure. A synthetic universal proposition must be eliminated simultaneously with the acceptance of a negative instance; otherwise, contradiction in science would result. It follows that if we 'save' a law when a prediction in terms of it is not fulfilled, we do not regard such a law as a synthetic universal proposition (empirical law), but rather as a procedural rule in terms of which 'warranted prediction' is defined (theoretical law). The difference between empirical laws and theoretical laws is one between different propositional meanings and is therefore not defined in terms of rules of procedure, but it becomes apparent by analysis of the rules. This distinction is closely related to the question whether laws are conventions.
14. 'Cause' and 'effect' are defined in terms of 'law.' Failure to realize that the meaning of 'cause' implies the meaning of 'law' leads to the idea that the effect is contained in the cause, that there is a necessary relation between cause and effect. Hume has shown that causal relations are external relations, but he did not perform a logical analysis of external relations. This issue too has been obscured by failure to distinguish between deductive logic and the logic of empirical procedure.
15. The principle of causality is not the most general causal law. It is no law at all, since no predictions can be made in terms of it, but rather a declaration of the resolution not

to renounce the search for causes in any instance and of the belief that this search will not be in vain. Similar observations apply to the principle of the uniformity of nature.

16. If we call empirical knowledge 'merely probable' in contrasting it with 'absolutely certain' rational knowledge, we do not give the term 'probable' the meaning (or, rather, meanings) it has *within* empirical science. This contrast, often erroneously interpreted as relating to different intensities of belief, is one between reversible and irreversible knowledge. The result of a correct logical deduction or mathematical proof is 'eternally valid,' whereas the result of a correct decision in empirical science is subject to permanent control, and hence may have to be reversed.

When the term 'probability' is used in statistical theory, its frequency interpretation is adequate. Probability laws are procedural rules of higher order that 'impose conditions' upon theoretical laws concerning relative frequencies within finite series of events. The chief issue of modern probability analysis is the relation of this meaning of the word to the meaning it has when we speak of the probability of single events or of scientific hypotheses, where it refers to what is called 'degree of confirmation' or 'weight of evidence.' This issue is one concerning specific rules of scientific procedure (rules of probability preference). These rules need not contain implicit reference to statistical laws, but their correlation with statistical laws is always possible and, moreover, a methodological desideratum.

17. A clear distinction between relations of meanings and matters of fact, or more precisely, between issues of pure deductive logic and issues of empirical procedure, is also required in an analysis of the relation of biology and psychology to physics. The failure to make this distinction has obscured the controversy between mechanists and vitalists as well as the treatment of the psycho-physical problem. The logical problem: Are biological or psychological terms definable by physical terms? and the empirical problem:

Are the phenomena of life and mind explainable in terms of established physical laws? must be well distinguished. A negative answer to the first question precludes an affirmative answer to the second, but an affirmative answer to the first does not imply an affirmative answer to the second. The answer to the first question is affirmative regarding biology, but negative regarding psychology. The answer to the second question is negative for both psychology and biology.

18. The history of philosophy reveals a close correspondence between the theory of knowledge and value theory, particularly ethics, and this correspondence has its source in the structure of the problems. The theory of knowledge seeks to clarify the criteria of correct beliefs; the theory of value, the criteria of correct valuations. The 'objectivistic' interpretation of value as a property of objects corresponds to the 'objectivistic' interpretation of truth as a property of propositions; the 'subjectivistic' interpretation of value as a product of emotional attitudes corresponds to the 'subjectivistic' interpretation of truth as a product of thinking. Both types of interpretations are inadequate, the former because it disregards the implicit reference to human spontaneity, the latter because it disregards implicit reference to criteria of correctness. When these criteria—the axiological rules and the procedural rules respectively—are made explicit, it is seen that the value judgment 'A valuation of a given object is correct in terms of a presupposed system of axiological rules by virtue of the properties of the object' has a form corresponding to that of the proposition 'A scientific decision concerning a given proposition *p* is correct in terms of a given system of procedural rules on the basis of accepted propositions.' Propositions of both kinds are analytic propositions. By misinterpreting value judgments as synthetic propositions of a particular kind and contrasting them with fact-statements, one is led to suppose a realm of values besides the realm of facts or existing objects. But

the contrast between fact and value is not one between different realms of being, but between two different types of rules, namely, procedural rules and axiological rules. To different realms of being, different fundamental meanings would correspond, but no peculiar fundamental meanings can be found in the axiological rules.

19. Most methodological issues in social science are directly or indirectly concerned with some aspects of the relation between natural science and social science, particularly the extent to which the methods of the former are appropriate to the latter. However, the nature of these methods is frequently misinterpreted, and this misinterpretation has often suggested erroneous views concerning the range of their applicability; moreover, it is often maintained that the exclusive appropriateness of a certain method of social science can be demonstrated on *a priori* grounds. But this is possible only if 'social science' is defined in terms of methods of inquiry or if a particular branch of social science is characterized in this way.
20. The issue between behaviorists and introspectionists is a significant example. The 'real' issue concerns the role of general assumptions about psychical processes that are or ought to be made in social science. It is obscured, on the one hand, by the behaviorists' thesis that the results of introspection are uncontrollable and therefore unscientific, and, on the other hand, by their opponents' thesis that introspection can yield self-evident irrefutable knowledge of human behavior.
21. Methodological, genetic, and axiological relations and relations of meanings are frequently confounded in discussions concerning the nature of society. In the controversies between universalistic and individualistic doctrines and between organicist and mechanist doctrines, the claim has been made time and again that adequate methods of social research and norms for social actions can be deduced from hypotheses about the origin of social life or from the mean-



ing of social relations. Accordingly, logical priority and genetic priority have not been properly distinguished. As soon as these distinctions are made, a number of apparently conflicting views regarding sociological methods will no longer be regarded as irreconcilable.

The results of Max Weber's analysis of the basic sociological terms can be accepted with slight modifications.

22. In speaking of the objective meaning of a sign, we refer to a meaning attributed to it by a rule generally accepted within a social group. It is important to recognize the relativity of 'objective meaning of signs' to given rules.
23. Comparison of physical laws and social laws has been misled by erroneous preconceptions concerning the nature of the former. Physical laws have been contrasted as necessary or strict laws with mere rules or tendencies prevailing in the social field. But as soon as it is realized, first, that no synthetic proposition is necessarily valid and, second, that many physical laws, among them the most general laws, are not strict (empirical) laws either, this contrast is seen to be of no fundamental methodological significance. The issue has been obscured by lack of a sharp distinction between pure mathematics and applied mathematics and by the ambiguity of the conception of free will.
24. The deterministic thesis that every event in the psycho-physical as well as in the physical realm is governed by causal laws, and the opposite thesis that free will sets limits to causality in the psycho-physical realm are declarations of methodological resolutions. They are usually misinterpreted as ultimate justifications of such resolutions. The thesis of free will has been invoked to justify on ultimate grounds very different, and even conflicting, methodological resolutions.
25. It is declared that subjective factors enter into the social sciences, which, for this reason, do not yield objective knowledge. One group of social scientists and social philosophers holding this view maintains that subjective factors

can be eliminated by adopting the methods of natural science; another group, that the subjectivity is ineradicable. We have shown that this issue is linked to a conception of objective validity that is not in accordance with scientific procedure. Closer analysis reveals that 'objective validity of synthetic propositions' must be defined in terms of rules of empirical procedure. It is then realized that the elliptical formulation of problems related to the concept of objectivity is largely responsible for the controversies concerning the objectivity of social science.

26. One more persistent methodological issue in social science that loses its point as soon as the meaning of 'objectivity' is clarified is that of the admissibility of value judgments. Since value judgments are analytic propositions, which is clearly recognized when their elliptical formulation is replaced by the complete formulation, they do not belong to the corpus of an empirical science. However, there is no objection to the acceptance into social science of sentences containing value terms provided their meaning is unambiguously established by axiological rules. It is then seen that there are no insoluble value problems.
27. In determining the relation between social theory and social practice, we have again to beware of confounding causal relations with logical relations. Practical goals are in many instances causal conditions for the setting of theoretical goals, but the meaning of 'theoretical goal of inquiry' is logically prior to the meaning of 'practical goal of inquiry.'
28. Applying the distinction between empirical laws and theoretical laws to an analysis of the fundamental laws of economic theory, we find that they are theoretical laws determining the direction of economic research. The thesis that they are indubitably true is one more instance of the confusion of matters of fact and relations of meanings.
29. The same fallacy is inherent in the traditional conception of rational economic behavior, including perfect foresight. But the problems of classical and neo-classical economic

theory can be formulated without reference to this notion. The chief critical objections to economic theory are sound in so far as they are directed against its aprioristic interpretation, but these objections do not seriously affect its basic method.

Some remarks concerning the bearing of our results on doctrinal controversies in philosophy may be added. As to the issue between rationalists and empiricists, it is seen that those empiricist doctrines which do not claim that indubitable truth is attained by immediate experience are basically right in their interpretation of empirical procedure. They are also right in rejecting the claims made by extreme rationalists that irrefutable statements about reality may be established by pure reason. But they are wrong in supporting nominalistic theories of abstraction and in attempting to define 'meaning' in terms of truth-conditions. Moreover, they often fail to give much attention to the fundamental philosophical problem of clarification.

The issue between realists and idealists concerns the meaning of 'reality of physical objects.' The idealists are right in declaring that it must be defined in terms of human experience and in rejecting the attempts of 'naive' realists to explain knowledge of the external world in terms of effects on the sense organs brought about by things-in-themselves. Moreover, they are right in emphasizing—as Kant and the Neo-Kantians did—that the unity of a physical cosmos is an ideal of inquiry, and, accordingly, not 'given,' but 'set as a task.' But those idealists are wrong who claim that the existence of the physical world depends upon actual processes of thought, that there could not be a physical world without beings capable of perception and thought. They are also wrong in proposing a definition of 'reality of the external world' in terms of *actual* experience of finite beings. We cannot deal here with idealistic theories (e.g. those of Malebranche or Berkeley) that define 'reality of the external world' in terms of God's Infinite Mind.

In the controversy between monists and dualists the latter are

right in holding that psychological terms and physical terms are irreducible to one another. But this does not mean that there can be no causal relations between physical events and psychical events. The monists are right in emphasizing this interdependence and in suggesting its scientific investigation.

The preceding remarks do not suggest that these philosophical issues can be completely settled by logical analysis. We hold, rather, that phenomenological problems are involved that are not within the scope of logical analysis, which presupposes fundamental objective meanings as 'constituted.' But those aspects of the doctrinal struggles that have always been and still are in the foreground of discussion do fall within its scope. These discussions refer either to relations among conceptual or propositional meanings irrespective of issues of empirical validity, or to the criteria of empirical validity.

The relation between methodology and science—already referred to in Chapter III—is more readily understood than the place of methodology within the whole framework of philosophical analysis. Methodology does not speak 'about' empirical science in the same sense as empirical science speaks about the world; it rather clarifies the meaning of 'empirical science.' Hence it cannot be strictly separated from empirical science, for the ideal of clarity is *inherent* in the meaning of 'scientific knowledge.' Scientific progress may indeed be achieved on different levels of clarity, and there may be disagreement among scientists about how much effort should be devoted to the task of clarification. But the history of science provides ample evidence that thorough analysis of methods and fundamental concepts may be of the greatest moment for the advancement of knowledge. This holds particularly in the field of social research, where the laws in terms of which predictions are warranted are seldom explicitly formulated.

We have already mentioned this point in Chapter XIV, but it deserves further emphasis. If we are challenged to offer grounds for the prediction that a particular body dropped in empty space from a roof sixty-four feet above ground will need about two

seconds to fall to the ground, we shall be able to declare without hesitation that this prediction is warranted in terms of Galileo's law of falling bodies. But things are different with most predictions in social science.

Let us suppose a bill is introduced in Congress, and an 'insider' predicts that it will be passed. Asked to offer grounds for his prediction, he might say, 'The Democratic Party will vote *en bloc* in favor of this bill, and there will be, moreover, a considerable number of Republicans who will support it. The result will thus be a vast majority for the bill.' But this is not to offer grounds for the prediction; it is rather to replace it by a more precise prediction. To offer grounds implies reference to empirical or theoretical laws. Let us now suppose that the person making the prediction is induced by application of a sort of Socratic method to formulate the laws underlying his prediction. If we look into a handbook of political science in order to determine whether they are established laws, we shall hardly find them. Generally speaking, one does not find in a handbook of sociology or political science a system of laws such as one may expect to find in a handbook of physics, chemistry, or biology. It is different with economics, but most predictions of economic events are not exclusively in terms of economic laws, and the other social laws involved are seldom explicitly formulated.

The difference between the levels of clarity attained in natural science and in social science is not satisfactorily explained by pointing to the fact that social events are interrelated in a more complex way than physical events. This might be accepted as a tentative preliminary answer if the issue at stake were to explain why predictions in natural science are more often successful than predictions in social science. However, we are not concerned with the success of predictions but with their foundations. The social scientist who declares a certain prediction to be warranted must be able to substantiate this claim by referring to the laws in terms of which it is warranted. No acquisition of additional knowledge is required for this purpose but only clarification of the presupposed theoretical laws. It must be ad-

mitted that clarifying implicit assumptions of higher structural complexity is more difficult, but this alone cannot explain why the issue has hardly ever been faced squarely. A clue to the explanation is provided by the prevailing differentiation between genuine laws in natural science and 'mere rules' in social science. Whether these rules are regarded as merely temporary expedients to be dropped as soon as genuine social laws have been found, or whether it is held, on the contrary, that there can be no genuine laws in social science (since human will is free), an inferior status is in either case allotted to them that seems to make them unworthy of rational analysis. Once this idea has been discarded, the task of making explicit the implicit theoretical laws in social science and their interrelation with one another and with physical laws can no longer be neglected. It is an arduous task, but one that can be undertaken with confidence and zeal as soon as we have rid ourselves of traditional misconceptions.

It would therefore be appropriate to start with a systematic attempt to eliminate these errors from social science. A survey would have to be made of the actual methodological controversies in the different social sciences, and the arguments offered in support of each of the conflicting doctrines would have to be examined. These arguments must be classified according to their formal structure. The methodological scheme outlined in Part I of this book gives a preliminary idea of how this should be done, but it is, of course, in need of being elaborated in much greater detail. The end-in-view is to determine precisely the procedural significance of all controversial points.

This applies particularly to issues of terminology. Methodological analysis is not concerned with the physical aspects of terms but with their meanings, and the problem is whether a proposed definition of a term is in accordance with its actual usage in scientific procedure. If there are different meanings attached to it, and if each meaning is related to a particular method, then the struggle over words is indicative of disagreement about the methods to be applied. To substantiate the claim

for the preferability of one method over the other, we have to refer to presupposed rules of procedure. A thorough analysis of equivocal terms will often lead to the core of the methodological issues.

One of the most important consequences to be expected from methodological analysis is the removal of obstacles that aprioristic and relativistic fallacies have put in the way of co-operation between social scientists. The apriorist as well as the relativist is prone to disregard objections raised against his arguments by fellow-scientists: the former because he believes himself to be in possession of self-evident truths, the latter because he is frequently more concerned with the extra-theoretical motives 'behind' the arguments of his opponent than with their content.

We shall conclude with some remarks concerning scientific co-operation and its relation to methodology.

Successful co-operation in science involves planned co-ordination of different kinds of accomplishments in science. Such co-ordination requires insight into the extremely complicated structure of their relationship. It is one of the objectives of methodological analysis to attain this insight.

If we first consider the relations between inquiries in different fields of science, we must bear in mind that inquiry in one field can be significant for inquiry in another not only by providing results that it has achieved but also by providing methods that it has employed. Frequently, however, such methods are not adopted unmodified; there is greater or lesser variation, in which certain formal principles remain invariant.

Other forms of scientific co-operation are recognized when one considers the various stages of scientific inquiry: the setting of problems, the treatment of problems, the solution of problems. It often happens that one scientist states the problem, another starts the investigation, and still another—basing his work, perhaps, on the preceding investigations of generations of scientists—succeeds in solving it. This is so obvious that it seems hardly in need of being mentioned. If, by a thorough analysis of the

more important types of problems and methods, their logical structure and the range of applicability of the pertinent hypotheses were clarified, the appraisal of their presumable relevance for other domains of inquiry would be less dependent upon the inspirations of great scientists than it is at present.

Various types of investigations may be distinguished, to which various types of co-operation correspond. One group of scientists collects data that are then interpreted by another group. The hypotheses formulated by the latter are controlled by the observations of other scientists, and so on in a potentially endless process. Control may lead to objections to proposed results or methods, i.e. to criticism.

Criticism is one of the most important forms of scientific co-operation.<sup>1</sup> To label the activity of the critic as 'destructive' is misleading. By showing a person that he has departed from the right path, we help him and all who follow him; we contribute to their progress in the direction of their goal. Invalidation no less than verification of propositions is progress in inquiry.

Yet criticism may be destructive, i.e. detrimental to scientific progress, when the scope of an objection is overrated. The critic may then be compared to a surgeon who, in operating, removes more than is necessary. A surgeon with a thorough knowledge of the anatomy and the physiology of the human body will seldom commit such an error, nor will a critic who has a clear idea of the structure of scientific procedure be likely to err in this way. He will be able to determine precisely the rules violated and the type of violation. It may be assumed that methodology will substantially contribute to the progress of social science by making explicit the implicitly presupposed standards of scientific criticism.

But methodology should not be evaluated exclusively in terms of its direct contribution to scientific progress. It fulfils a most important social function in promoting mutual understanding among scientists, particularly social scientists. The power of argument rests on this understanding; and when the foundation is too weak, it is the argument of power that triumphs.



## Notes

### CHAPTER I

1. It should be noted, however, that mathematical knowledge (*dianoia*) is to Plato not the highest type of knowledge. The first place in the hierarchy is occupied by the pure intuition of Ideas or Forms (*noësis*). Cf. Aristotle, *Metaphysics*, transl. by W. D. Ross, I. 6: 'Further, besides sensible things and Forms he says there are the objects of mathematics, which occupy an intermediate position, differing from sensible things in being eternal and unchangeable, from Forms in that there are many alike, while the Form itself is in each case unique.'
2. An excellent historical analysis of Galileo's ideas about the nature of physical laws may be found in A. Koyré's *Etudes Galiléennes* published in the series 'Histoire de la pensée' Actualités scientifiques et industrielles, Paris, 1939. There are three interconnected studies, 'A l'aube de la science classique' No. 852, 'La loi de la chute des corps Descartes et Galilée' No. 853, 'La loi de l'inertie' No. 854.
3. *The Mathematical Principles of Natural Philosophy*, transl. by Andrew Motte (1729), Bk I, Scholium: 'Absolute, true, and mathematical time, of itself, and from its own nature flows equably without regard to anything external, and by another name is called duration . . .'  
'Absolute space, in its own nature, without regard to anything external, remains always similar and immovable.'  
'Absolute motion is the translation of a body from an absolute place into another . . .'
4. *Opere*, ed. Alberti, VII, p. 355.
5. Essay II, 8, 15.
6. *Principles of Human Knowledge*, Introduction, §§ 13-16.

## CHAPTER II

1. The most elaborate criticism of the nominalistic theories of abstraction may be found in E. Husserl's *Logische Untersuchungen*, 3rd ed., Halle, 1922, Vol. II, pp. 137-215. It is aptly summarized in M. Farber, *The Foundation of Phenomenology*, Cambridge, 1943, Ch. IX.
2. An interesting analysis of the philosophical connotations of the terms 'proposition,' 'sentence,' 'statement,' 'judgment' is given by H. R. Smart, 'The Unit of Discourse,' *Philosophical Review*, Vol. L, pp. 268-88.
3. Essay II, 1, 2. Locke's simile may be traced back to that of the wax block in Plato's *Theaetetus*, 191D.
4. Cf. Preface to *Nouveaux essais sur l'entendement humain*.
5. Cf. E. Husserl, *Formale und transzendente Logik*, Halle, 1929, and his posthumous work *Erfahrung und Urteil, Untersuchungen zur Genealogie der Logik*, edited by L. Landgrebe, Prague, 1939, and the Preface to the English translation (entitled *Ideas*) of his *Ideen zu einer reinen Phaenomenologie und phaenomenologischen Philosophie*, by W. R. Boyce Gibson, New York, 1931.  
I have discussed this point in my articles 'Phenomenology and Logical Empiricism' in *Philosophical Essays in Memory of Edmund Husserl*, edited by Marvin Farber, Cambridge, Mass., 1940, pp. 143-64, and 'Strata of Experience' in *Philosophy and Phenomenological Research*, Vol. I, pp. 313-24.
6. Cf. Husserl, *Logische Untersuchungen*, Vol. II, pp. 225 ff., and Farber, *The Foundation of Phenomenology*, Ch. X.
7. 'Frame of possibilities' or 'possible world' or 'totality of synthetic propositions' are used as synonyms. To say that a synthetic proposition restricts the frame of possibilities is tantamount to saying that it negates other synthetic propositions. 'Omnis determinatio est negatio.'
8. *De Interpretatione*, transl. by E. M. Edghill, I, 4.
9. A number of interesting analyses concerning the meaning of imperatives have appeared in recent years. The following may be mentioned: Jørgensen, 'Imperatives and Logic,' *Erkenntnis*, Bd. 7, Heft 4, pp. 283-96; K. Menger, 'A Logic of the Doubtful, On Optative and Imperative Logic,' *Reports of a Mathematical Colloquium*. Second Series, Issue I, pp. 53-64; A. Hofstadter and J. C. C. McKinsey, 'On the Logic of Imperatives,' *Philosophy of Science*, Vol. VI, No. 4, pp. 446-57.

10. The prevailing interpretation of truth-relations is that a statement of truth-relations is a conditional statement concerning the truth (or falsity) of propositions and that it is therefore not required to investigate whether any of the propositions involved is actually true (false). But the point is that no reference to 'truth' is implied by the truth-relations of synthetic propositions. This point will be further clarified by the analysis in Chapter IV, where it is shown that 'truth of synthetic propositions' must be defined in terms of the rules of scientific procedure.
11. Let  $p$  and  $q$  be two propositions, T and F the abbreviations of 'true proposition' and 'false proposition,' and let  $p \wedge q$  stand for ' $p$  and  $q$ ,'  $p \vee q$  for ' $p$  or  $q$ ,' and  $p \rightarrow q$  for 'if  $p$  then  $q$ .' We may then construct the following truth-table:

$p$	$q$	$p \wedge q$	$p \vee q$	$p \rightarrow q$
T	T	T	T	T
F	T	F	T	F
T	F	F	T	F
F	F	F	F	T

If we replace ' $p$  is true' by  $p$  and ' $p$  is false' by non  $p$ , and do the same with  $q$ , we can formulate these truth-relations without any reference to the term 'truth.' It will suffice to show this for the case of two alternative propositions, but it applies to any number of propositions and to conjunction and implication as well. To say that  $p \vee q$  must be true if (a)  $p$  is true and  $q$  is true, (b)  $p$  is true and  $q$  is false, (c)  $p$  is false and  $q$  is true, and that  $p \vee q$  must be false if (d)  $p$  is false and  $q$  is false, is tantamount to saying that  $p \vee q$  is logically entailed in (a)  $p \wedge q$ , (b)  $p \wedge$  non  $q$ , (c) non  $p \wedge q$ ; and that non ( $p \vee q$ ) is logically entailed in (identical with) (d) non  $p \wedge$  non  $q$ .

12. See footnote 6.
13. *Appearance and Reality*, second ed., 9th impression, Oxford, 1930, p. 27; cf. pp. 21-9 and pp. 512-38 (Appendix B). One of Bradley's basic points is briefly formulated in the footnote on p. 27: 'The relation is not the adjective of one term, for, if so, it does not relate. Nor for the same reason is it the adjective of each term taken apart, for then again there is no relation between them. Nor is the relation their common property, for then what keeps them apart? They are now not two terms at all, because not separate.'
14. *Philosophical Essays*, New York, 1910, pp. 161 ff.
15. Perhaps the most important contributions to this discussion are those by G. E. Moore, 'External and Internal Relations,' in *Philo-*

*sophical Studies*, London, 1922, pp. 276-309; and W. E. Johnson in his *Logic*, Vol. I, Cambridge, 1921. He says (p. 250), 'I hold, then, that relations between adjectives as such are internal; and those between existents as such are external. In this account, adjectives are to include so-called external relations, even the characterizing relation itself, as well as every other relation. The otherness which distinguishes the "this" from the "that" is the primary and literally the sole external relation, being itself direct and underived. And this relation is involved in every external relation.' If we replace in this quotation 'existents as such' by 'valid propositions *qua* valid' we come close to one of the interpretations of the meaning of 'external relations' offered in the text. Cf. also two recent articles in the *Philosophical Review*, viz. F. L. Will, 'Internal Relations and the Principle of Identity,' Vol. XLIX, pp. 497-514, and R. W. Church, 'Bradley's Theory of Relations and the Law of Identity,' Vol. LI, pp. 26-46.

16. The first systematic analysis of the nature of relations is offered in Aristotle's *Categoriae*, 7: 'Those things are called relative, which, being either said to be *of* something else or *related to* something else, are explained by reference to that other thing. For instance, the word "superior" is explained by reference to something else, for it is superiority *over something else* that is meant.' The issue of the objectivity of relations is raised, but Aristotle fails to lay down a definite opinion on this point, and the Aristotelians and Neo-Platonists were divided among themselves in this respect. Plotinus and Boëthius expressed the subjectivistic point of view; Avicenna and Aquinas hold an intermediate position stating that relations, while products of the mind, have a foundation in things. The latter declares that '*relatio fundatur in aliquo sicut in causa*' (4 sent. 27).

Locke declares: '. . . relations having no other reality but what they have in the minds of men, there is nothing more required to this kind of ideas to make them real, but that they be so framed, that there be a possibility of existing conformable to them. These ideas themselves, being archetypes, cannot differ from their archetypes, and so cannot be chimerical, unless any one will jumble together in them inconsistent ideas.' *Essay*, II, 30, 4.

Leibniz, in whose system the concept of relation occupies a central place, declares, 'Les relations ont une réalité dépendante de l'esprit . . . mais non pas de l'esprit de l'homme puisqu'il y a une suprême intelligence, qui les détermine toutes en tout temps.' *Nouveaux Essais*, II, 30, 4. This view is connected with his distinction between possibility (*vérités de raison*) and compossibility (*vérités de fait*).

Hume divides relations 'into two classes; into such as depend entirely on the ideas, which we compare together, and such as may be chang'd without any change in the ideas. 'Tis from the idea of a triangle, that we discover the relation of equality, which its three angles bear to two right ones; and this relation is invariable, as long as our idea remains the same. On the contrary the relations of *contiguity* and *distance* betwixt two objects may be chang'd merely by an alteration of their place, without any change on the objects themselves or on their ideas; and the place depends on a hundred different accidents, which cannot be foreseen by the mind.' *Treat.* I, III, 1.

17. Cf. Carnap, *Logical Syntax of Language*, New York, 1937, p. 4: 'By a *calculus* is understood a system of conventions or rules of the following kind. These rules are concerned with elements—the so-called *symbols*—about the nature and relations of which nothing more is assumed than that they are distributed in various classes. Any finite series of these symbols is called an *expression* of the calculus in question. The rules of the calculus determine, in the first place, the conditions under which an expression can be said to belong to a certain category of expressions; and, in the second place, under what conditions the transformation of one or more expressions into another or others may be allowed.'

### CHAPTER III

1. *Geometrie und Erfahrung*, Berlin, 1921, p. 3 f. The wording in German is: 'Insofern sich die Sätze der Mathematik auf die Wirklichkeit beziehen, sind sie nicht sicher, und insofern sie sicher sind, beziehen sie sich nicht auf die Wirklichkeit.'
2. New York, 1927; cf. also his later work, *The Nature of Physical Theory*, Princeton, 1936.
3. The first precise definitions of this concept were given by K. Menger and P. Uryson in 1922. Cf. the short historical survey in Menger's *Dimensionstheorie*, Leipzig u. Berlin, 1922, pp. 83 ff.
4. This was achieved in Felix Klein's 'Erlanger Programm' (1872), reprinted in F. Klein's *Gesammelte mathematische Abhandlungen*, Vol. I, pp. 460 ff., Berlin, 1921.
5. *Collected Papers*, ed. by C. Hartshorne and P. Weiss, Cambridge, Mass., 1931 ff., Vol. I, p. 55.
6. We shall understand by 'methodology' logical analysis of the rules of scientific procedure and shall use the term 'logic' in a broader

sense, comprising deductive reasoning in the strict sense as well as analysis of scientific procedure.

7. The suggested definitions accord well with the conception of science as a self-correcting process. It must, however, be borne in mind that the logician is not concerned with the description of the process as a temporal event but with the analysis of the rules 'governing' it.

#### CHAPTER IV

1. Cf. Dewey, *Logic. The Theory of Inquiry*, New York, 1938, p. 16: 'To engage in an inquiry is like entering into a contract. It commits the inquirer to observance of certain conditions. A stipulation is a statement of conditions that are agreed to in the conduct of some affair. The stipulations involved are first implicit in the undertaking of inquiry. As they are formally acknowledged (formulated), they become logical forms of various degrees of generality. They make definite what is involved in a demand.'
2. In the history of philosophy up to Leibniz this principle was usually identified with what we call today the 'principle of causality.' In this form it may be traced back to Heraclitus and Plato (e.g. *Tim.* 28). Aristotle regards it as essential for wisdom to tell us the 'why' of anything (*Met.* i. 1. 981b).

Descartes offers the following formulation: 'Nulla res existit, de qua non possit quaeri, quatenus sit causa, cur existat' (*Resp.* ad ii object. ax. 1).

Spinoza's formulation (*Eth.* i, prop. viii) and those given by various other rationalistic philosophers in the seventeenth and eighteenth centuries are close to that of Descartes.

Leibniz regards it as the basic principle of *vérités de fait* (while the principle of contradiction is the basic principle of *vérités de raison*), and (interpreting it teleologically) makes it a cornerstone of his philosophical system. But his disciple Christian Wolff attempted to deduce it from the principle of contradiction by declaring: 'It would be contradictory to presume that something can come from nothing.' Cf. *Philosophia prima sine ontologia*, 1737, §§ 60 and 70. This argument is untenable.

Kant excludes any transcendent interpretation of our principle. It is according to him 'der Grund moeglicher Erfahrung, naemlich der objektiven Erkenntnis der Erscheinungen, in Ansehung des Verhältnisses derselben in der Reihenfolge der Zeit,' *Kr. d. r. Vern.* 2. Anal. d. Erf.

The distinction between the metaphysical principle of sufficient

reason and the *logical* principle of sufficient reason, 'dass wir ohne Grund nichts fuer wahr halten koennen und sollen,' is made in I. G. H. Feder's *Logik und Metaphysik*, 1788, p. 269. The methodological character of the principle is also emphasized in J. F. Fries' *System der Logik*, 3rd ed., 1837, pp. 177 f.

One of the most widely known treatises dealing with the principle of sufficient reason is Schopenhauer's *Ueber die vierfache Wurzel des Satzes vom zureichenden Grunde*. Cf. also W. M. Urban, 'The History of the Principle of Sufficient Reason. Its Metaphysical and Logical Formulations,' *Princeton Contributions to Philosophy*, ed. by A. T. Ormond, Vol. 1, No. 1, Princeton, 1898.

3. The term *Protokollsatz* was introduced by O. Neurath; cf. 'Sociologie im Physikalismus,' *Erkenntnis* 2, 1931, 'Protokollsätze,' *Erkenntnis* 3, 1932, and then widely accepted among the logical positivists. A history of the usage of this term may be found in C. G. Hempel's 'On the logical positivist's theory of truth' (*Analysis* 11, 49 ff.). Its interpretation has been strongly tinged by the physicalistic elements in logical positivism. It is free of these connotations when used in this book.
4. The concept of situation is one of the basic concepts in Dewey's methodology. 'Its import may perhaps be most readily indicated by means of a preliminary negative statement. What is designated by the word "situation" is *not* a single object or event or set of objects and events. For we never experience nor form judgments about objects and events in isolation, but only in connection with a contextual whole. This latter is what is called a "situation." (Logic, p. 66.) The definition of 'inquiry' offered by Dewey is in terms of 'situation.' *'Inquiry is the controlled or directed transformation of an indeterminate situation into one that is so determinate in its constituent distinctions and relations as to convert the elements of the original situation into a unified whole.'* (Ibid. p. 104 f.) Cf. also Dewey's recent article 'Inquiry and Indeterminateness of Situations,' *Journ. of Philosophy*, Vol. xxxix, pp. 290-96, in reply to D. S. Mackey's article 'What does Mr. Dewey mean by an "Indeterminate Situation"?' *ibid.* pp. 141-8.

Now it seems to me that the concept of 'scientific situation' as defined in the text contains those very elements of Dewey's concept which are relevant in a logical analysis of scientific procedure.

5. The methodological significance of the concept of falsification is duly emphasized in K. Popper's remarkable book: *Logik der Forschung. Zur Erkenntnistheorie der modernen Naturwissenschaft*, Vienna, 1935. I am in various respects indebted to this work, par-

- ticularly to the remarks on the use of the terms 'true' and 'confirmed' (*wahr und bewahrt*), pp. 203 f., for the suggestion that falsifiability should be regarded as the criterion of empirical propositions (p. 12 ff) and for the analysis of probability (pp. 94-153). However, I cannot accept Popper's theory of basic propositions, which is essential for his approach.
6. Cf. Husserl's distinction between 'objektiven Ausdruecken' and 'wesentlich subjectiven und okkasionellen Ausdruecken,' *Logische Untersuchungen*, Vol. II, pp. 79 ff.
  7. Cf. P. Frank, *Between Physics and Philosophy*, Cambridge, Mass., 1941, 151-71.
  8. *De interpretatione*, 9. This analysis later played a role in the controversy between determinists and indeterminists. Cf. H. Scholz, *Geschichte der Logik*, Berlin, 1931, pp. 33 f. and 75 ff. The issue of decidability in empirical science must be strictly separated from that of decidability in mathematics and its relation to the principle of the excluded middle as discussed by L. E. J. Brouwer and K. Goedel. For an analysis of Brouwer's point cf. my *Das Unendliche in der Mathematik*, Vienna, 1930, pp. 58-68 and 187 ff.
  9. Cf. W. James, *Pragmatism*, New Impression, New York, 1908, pp. 222 f. "The "absolutely" true, meaning what no farther experience will ever alter, is that ideal vanishing-point towards which we imagine that all our temporary truths will some day converge. It runs on all fours with the perfectly wise man, and with the absolutely complete experience; and, if these ideals are ever realised, they will all be realised together."

## CHAPTER V

1. Cf. Popper, *Logik der Forschung*, pp. 28 f.
2. Time appears as the independent variable in these differential equations. See e.g. P. Frank, *Das Kausalgesetz und seine Grenzen*, Vienna, 1932, pp. 142-6; and R. v. Mises, *Kleines Lehrbuch des Positivismus. Einführung in die empiristische Wissenschaftsauffassung*, The Hague, Holland, 1939, pp. 168 and 192 ff.
3. H. Poincaré has discussed the question of the unchangeability of laws in his study 'L'évolution des lois,' published in *Dernières Pensées*, Paris, 1913, pp. 5-32.
4. *Théorie analytique des probabilités*, seconde édition, Paris, 1814, Introduction, ii, iii. 'Une intelligence qui pour un instant donné, connaîtrait toutes les forces dont la nature est animée, et la situation respective des êtres qui la composent, si d'ailleurs elle était



assez vaste pour soumettre ces données à l'analyse, embrasserait dans la même formule, les mouvements des plus grands corps de l'univers et ceux du plus léger atome: rien ne serait incertain pour elle, et l'avenir, comme le passé, serait présent à ses yeux. L'esprit humain offre dans la perfection qu'il a su donner à l'astronomie une faible esquisse de cette intelligence. Ses découvertes en mécanique et en géométrie, jointes à celles de la pesanteur universelle, l'ont mis à portée de comprendre dans les mêmes expressions analytiques les états passés et futurs du système du monde.'

5. This did not last because the Huygens-Maxwell theory could not satisfactorily explain all phenomena of radiation. De Broglie's theory of corpuscular waves, an integral part of quantum physics, is a sort of synthesis of both views.

## CHAPTER VI

1. This issue can be traced back to the sentence in Henri Bergson's Introduction to the French translation of W. James' *Le Pragmatisme*, Paris, 1911, p. 11. 'On pourrait, ce me semble, résumer tout l'essentiel de la conception pragmatiste de la vérité dans une formule telle que celle-ci: *Tandis que pour les autres doctrines une vérité nouvelle est une découverte, pour le pragmatisme c'est une invention.*' This dictum, however, does not do full justice to the pragmatistic conception of truth and it does not consider the relation between discovery and invention. These cannot be contrasted as two opposites since 'invention' implies 'discovery.' Inventions are applications of discoveries and discoveries may be aimed at and evaluated with a view to such applications. This, however, is not essential in a logical analysis of the meaning of 'truth.' What matters here is the control of assertions by their application. The emphasis on this point is one of the most important contributions of pragmatism to the theory of knowledge.
2. *Elements of Logic*, 1825.
3. *A System of Logic*, 8th ed., 1885, p. 225.
4. Among his critics Charles Peirce stands out most prominently. Cf. *Collected Papers*, Vol. II, 1932, pp. 483-94.
5. *An Introduction to Logic and Scientific Method*, 1st ed., New York, 1934, p. 268.
6. Cf. his *Logic*, Ch. VIII.
7. *Determinismus und Indeterminismus in der modernen Physik. Historische und systematische Studien zum Kausalproblem*, Goeteborg, 1937, p. 104.

8. *Erkenntnis und Irrtum*, 2nd ed., 1906, p. 192 f. (These sentences are also quoted by Cassirer, *ibid.* pp. 103 f.)
9. The distinction between empirical laws and theoretical laws was suggested to me by Dewey's distinction between generic propositions and universal propositions (*Logic*, Ch. xiv) Cf. also the pertinent analysis in C. I. Lewis, *Mind and the World Order*, Cambridge, Mass., 1929, Ch. x, esp. pp. 334 ff.
10. Cf. A. Eddington, *The Philosophy of Physical Science*, New York, 1939, pp. 28 f.
11. Cf. my *Das Unendliche in der Mathematik und seine Ausschaltung*. Russell's *Introduction to Mathematical Philosophy*, New York, 1919, is still the best introduction to the subject.
12. Cf. D. Hilbert's *Foundations of Geometry*, transl. by F. J. Townsend, 2nd ed., Chicago, 1910, p. 3: 'Let us consider three distinct systems of things. The things composing the first system, we will call *points* and designate them by the letters A, B, C, . . . ; those of the second, we will call *straight lines* and designate them by the letters a, b, c, . . . ; and those of the third system, we will call *planes* and designate them by the Greek letters  $\alpha$ ,  $\beta$ ,  $\gamma$ , . . . We think of these points, straight lines, and planes as having certain mutual relations, which we indicate by means of such words as "are situated," "between," "parallel," "congruent," "continuous," etc. The complete and exact description of these relations follows as a consequence of the *axioms of geometry*'  
     Cf. also A. Tarski, *Introduction to Logic*, New York, 1941, pp. 120 ff., particularly p. 122.
13. Cf. Einstein, *Relativity. The Special and General Theory*, transl. by R. W. Lawson, New York, 1920, pp. 98 ff.
14. The problem of causality has always been one of the pivotal issues in philosophical speculation. Aristotle's *Metaphysics* with its doctrine of the four causes represents the peak of Hellenic speculation on this subject. For many centuries it held virtually undisputed sway over European epistemological thought. The most vigorous attack upon it was launched in Hume's *Treatise*. Hume has strongly influenced Kant's analysis in the *Critique of Pure Reason* and most of the pertinent epistemological and methodological controversies up to our own time.
15. A. Comte, C. Bernard, G. Kirchhoff, H. Hertz, and other scientists and philosophers pleaded for the elimination of the term 'causality' from scientific language. E. Mach proposed to replace it by the term ('mathematical') function.' Anglo-American discussions of this point have been frequently referred to by Karl Pearson's *Grammar*

of *Science*, first publ. London, 1892, where 'correlation' is suggested as a substitute for 'causality.'

16. Cf. the corresponding remarks about the principle of the uniformity of nature on p. 81 f.

## CHAPTER VII

1. New York, 1940.
2. Ibid. p. 362.
3. Ibid. p. 173.
4. Ibid. p. 173.
5. Ibid. p. 174.
6. Ibid. p. 174.
7. Cf. the two interesting articles: E. Nagel, 'Verifiability, Truth and Verification,' *Journ. of Phil.*, Vol. xxi, pp. 141-8; and C. I. Ducasse, 'Truth, Verifiability, and Propositions about the Future,' *Philosophy of Science*, Vol. 8, pp. 329-37.
8. Cf. Ch. iv, p. 53. This seems to contradict the following statement of C. I. Lewis (*Mind and the World Order*, p. 332): 'Moreover, a probable judgment, once true, is always true. A probability cannot change, because probability has no meaning except by relation to its premises or ground. New data do not invalidate the previous judgment, because they constitute a new problem and mark a new probability. The probable judgment based upon specific data is not only eternally valid, if it is ever valid, but if it is valid, it is absolutely and eternally true.'

But the incompatibility is only apparent; far from denying the reversibility of the acceptance of synthetic propositions Lewis stresses (as we do) that invalidation of a previously accepted proposition does not involve the judgment that the scientific decision by which it was accepted was incorrect.

9. London, 1921. The work contains also a brief survey of the history of probability philosophy and a large bibliography.

We shall not refer in the following analysis to the classical interpretation of probability as offered by Laplace. His fundamental concept of equipossible alternatives has been severely criticized by proponents of the frequency interpretation, particularly on the ground that 'equipossible' means 'equiprobable,' so that the definition of probability in terms of equipossible alternatives is circular. Furthermore it has been emphasized that not all probability statements are analyzable into a set of equipossible alternatives. Keynes, on the other hand, regards his theory rather as a modification of

the classical view than as a reversal of it. By his Principle of Indifference he seeks to remove the objections raised against the classical interpretation.

10. Op. cit. p. 3.
11. Ibid. p. 4.
12. Ibid. pp. 3 f.
13. Ibid. p. 3.
14. Ibid. p. 7.
15. Ibid. p. 10.
16. Ibid. p. 12.
17. Ibid. p. 13.
18. 'The classification of "primary" and "secondary" propositions was suggested to me by Mr. W. E. Johnson.' (*Ibid.* p. 11, footnote.) Cf. Johnson's *Logic*, Part I, pp. 165 ff.
19. It should be noted that the terms 'complete confirmation,' 'incomplete confirmation,' 'degrees of confirmation' are used with meanings different from those assigned to them in Carnap's Harvard Lectures on 'Testability and Meaning,' in *Philosophy of Science*, Vol. III, pp. 419-71; Vol. IV, pp. 2-40.  
 Cf. Carnap's argument on pp. 425 ff. and his definition of 'confirmable' (p. 456, which reads: 'A sentence S is called *confirmable* (or completely confirmable, or incompletely confirmable) if the confirmation of S is reducible (or completely reducible, or incompletely reducible, respectively) to that of a class of observable predicates.'
- In our terminology the terms 'complete confirmation' and 'verification' are synonymous, and the criterion of observability is not isolated from the other criteria of verification. 'Complete confirmation,' as we understand the term, is possible of synthetic universal propositions as well as of singular propositions.
20. *Probability, Statistics and Truth*, New York, 1939, p. 20.
21. Ibid. p. 22.
22. Ibid. p. 33.
23. Ibid. p. 33.
24. Ibid. p. 33.
25. Ibid. p. 308.
26. *Recherches sur la probabilité des jugements en matière criminelle et en matière civile*, 1837, ch. 5.
27. 'Principles of the Theory of Probability,' *Intern. Encyclopedia of Unified Science*, Vol. I, No. 6, Chicago, 1939, p. 52.
28. *Experience and Prediction*, Chicago, 1938, p. 314.
29. Ibid. p. 307.

30. Ibid. pp. 307 f.
31. Ibid. p. 318 f.
32. Ibid. pp. 319 ff. The full technical elaboration of Reichenbach's theory may be found in *Wahrscheinlichkeitslehre*, Leyden, 1935.
33. Ibid. p. 331.

## CHAPTER VIII

1. The ensuing brief outline of the conflicting arguments follows their apt presentation in B. Bavink, *The Natural Sciences; an Introduction to the Scientific Philosophy of To-day*, New York, 1932. I do not share, however, Bavink's view concerning these problems. Cf. also M. R. Cohen, *Reason and Nature*, New York, 1931, pp. 240 ff.
2. Cf. e.g. *The History and Theory of Vitalism*, transl. by C. K. Ogden, London, 1914.
3. Cf. R. Carnap, 'Die physikalische Sprache als Universalsprache der Wissenschaft,' *Erkenntnis* II, 432-65, pp. 449 f.
4. It was emphasized in medieval philosophy that nothing corporeal can be the cause of something incorporeal (events in the mind or soul). Cf. e.g. Augustine: 'Non putandum est, corpus aliquod agere in spiritum, quasi spiritus corpori facienti materiae vice subdatur.' (*Sup. genes. at lit.* XII) or Thomas Aquinas: 'Nihil corporeum imprimere potest in rem incorpoream' (*Sum. th.* I, 84, 6).  
Spinoza states: "Quae res nihil commune inter se habent, earum una alterius causa esse non potest" (*Eth.* I, prop. III).
5. Assistance of God is required to make this interaction possible.
6. 'Toute alliance de l'esprit et du corps qui nous est connue, consiste dans une correspondance naturelle et mutuelle des pensées de l'âme avec les traces du cerveau, et des émotions de l'âme avec les mouvements des esprits animaux' (*Rech.* II, 5).
7. The doctrine of parallelism appears here in the systematic context of his monadology and theory of preëstablished harmony. Soul and body are likened to two watches which have been so constructed that they must always show the same time. *Gerh.* IV, 498. The watch simile already appears in the writings of the Anti-Aristotelian Nic. Taurellus and of Geulincx.
8. Cf. *Wesen und Formen der Sympathie*, 2nd ed., Bonn, 1923, pp. 244 ff.

## CHAPTER IX

1. This metaphor appears in various of Plato's dialogues, e.g. in *Phaedrus*, 250.

2. Cf. P. Schrecker, 'La Méthode Cartésienne et la Logique,' in *Descartes Recueil* publié par 'La Revue Philosophique' à l'occasion du Troisième Centenaire du 'Discours de la Méthode,' Paris, 1937, pp. 336-67, particularly p. 363:

'Pour caractériser en peu de mots cette divergence, on pourra dire que, selon Descartes, la nécessité de certaines vérités est un épiphénomène qui marque celles que nous connaissons clairement et distinctement, tandis que, selon Leibniz, la clarté et la distinction de la connaissance sont un épiphénomène de la nécessité. Ou bien: pour l'un, ces vérités sont nécessaires parce que nous les connaissons clairement et distinctement; pour l'autre, nous les connaissons clairement et distinctement parce qu'elles sont nécessaires.'

3. We are not here concerned with the analysis of the meaning of valuation—to which, for example, R. B. Perry's *General Theory of Value*, New York, 1926, is chiefly devoted—but with the definition of 'value judgment' in terms of valuation.
4. In his *Principia Ethica*, Cambridge, 1903, p. 7, he writes: 'My point is that "good" is a simple notion, just as yellow is a simple notion; that just as you cannot, by any manner of means, explain to anyone who does not already know it, what yellow is, so you cannot explain what good is.' In a later article 'The conception of intrinsic value' in *Philosophical Studies*, London, 1922, Moore has modified his doctrine. He declares (p. 260), 'To say that a kind of value is "intrinsic" means merely that the question whether a thing possesses it, and in what degree it possesses it, depends solely on the intrinsic nature of the thing in question.' Cf. also the illuminating discussion in *The Philosophy of G. E. Moore*, Library of Living Philosophers, Vol. 4, Evanston, 1942.
5. Bergson makes the point that moral rules applied to relations within a closed society have a character fundamentally different from that of moral rules encompassing humanity as a whole. Accordingly, he rejects the view outlined in the text. Cf. *Les deux sources de la morale et de la religion*, Septième édition, Paris, 1932, Ch. 1.

## CHAPTER X

1. 'The Conflicting Psychologies of Learning. A Way Out,' *Psychological Review*, Vol. 42, pp. 491-516, p. 491.

## CHAPTER XI

1. A systematic analysis of the more important varieties of behaviorism may be found in A. F. Bentley's *Behavior, Knowledge, Fact*, Bloomington, Ind., 1935.
2. Cf. *Grundzuege der Psychologie*, I, p. 74: 'In dem vorgefundenen Objekt nennen wir psychisch, was nur einem Subjekt erfahrbar ist, physisch, was mehreren Subjekten gemeinsam erfahrbar gedacht werden kann.'
3. The incorporation of physicalism into the doctrine of logical empiricism is primarily due to O. Neurath. Cf. e.g. 'Physicalism,' *Monist* 41, 1931. 'Radikaler Physikalismus und wirkliche Welt,' *Erkenntnis* IV, 1934. 'Physikalismus und Erkenntnisforschung,' *Theoria* II, 97 and 234.
4. 'Les concepts psychologiques et les concepts physiques sont-ils fondamentalement différents?' *Revue de synthèse*, Tom X, No. 1, pp. 43-53, p. 44.
5. 'Analyse logique de la psychologie,' op. cit. pp. 27-42, esp. pp. 31-3.
6. *Encyclopedia of Unified Science*, The University of Chicago Press, Vol. I, pp. 42-62.
7. Ibid. p. 57.
8. Ibid. p. 59.
9. Ibid. pp. 58 f.
10. Ibid. p. 49 f.

## CHAPTER XII

1. *Soziologie, Untersuchungen ueber die Formen der Vergesellschaftung*, Leipzig, 1908; cf. p. 5: 'Ich gehe dabei von der weitesten, den Streit um Definitionen moeglichst vermeidenden Vorstellung der Gesellschaft aus: dass sie da existiert, wo mehrere Individuen in Wechselwirkung treten.'
2. *Wirtschaft u. Gesellschaft*, Part I, Tuebingen, 1925, p. 1.
3. Ibid. p. 13.
4. The translations are Talcott Parsons' in his *Structure of Social Action*, New York, 1937, pp. 640 f. and 649, which contains the best description and most thorough discussion in the English language of Weber's methodological views. The bibliography added to the book contains a large number of the more important contributions to the subject published up to 1936. I have discussed some of the pertinent points, particularly Weber's theory of ideal types, in more detail in my *Methodenlehre der Sozialwissenschaften*.

5. 'Der Gegenstand der reinen Gesellschaftslehre,' *Archiv f. Sozialw.*, Vol. 54, pp. 329 ff.
6. *Der sinnhafte Aufbau der sozialen Welt. Eine Einleitung in die verstehende Soziologie*, Vienna, 1932, pp. 161 ff.

## CHAPTER XIII

1. Many of the basic points made in the philosophical controversies on freedom of the will may be found in the third book of Aristotle's *Nicomachean Ethics*. Aristotle's view influenced (in different ways) the ethical doctrines of the Stoics (Seneca, Epictetus), Epicureans (Lucretius), and the Neo-Platonists (Plotinus), and—directly or indirectly—medieval speculation. Here the problem is linked with that of theodicy and the dogma of original sin (Augustine). Among the most prominent adherents to a doctrine of free will are Maimonides, Duns Scotus, and William of Occam. Thomas Aquinas and other Schoolmen tried to reconcile metaphysical determinism with psychological indeterminism. The three founders of Protestant creeds, Luther, Zwingli, and Calvin, were determinists. Examination of the contributions to the issue of free will in modern philosophy from Hobbes and Descartes to Bergson and James discloses the great variety of problems treated under this name.
2. This is the rationalistic view. The voluntarists hold that free will is not entirely determined by reason. Cf. e.g. Duns Scotus, *Nihil aliud a voluntate est causa volitionis in voluntate* (I. sent. 2, d. 25, qu. 1).
3. A good survey of the literature related to this subject may be found in G. A. Lundberg, *Foundations of Sociology*, New York, 1939, Notes to Ch. II, pp. 77-88.
4. Cf. e.g. R. A. Fisher, *Statistical Methods for Research Workers*, Edinburgh and London, 1925; and I. Neyman, *Lectures and Conferences on Mathematical Statistics*, Washington, D. C., 1938.
5. *Die Grenzen der naturwissenschaftlichen Begriffsbildung*, 5. Aufl. Tuebingen, 1929.
6. *Geschichte und Naturwissenschaft*, Strassburg, 1904, republished in *Praeludien*, Tuebingen, 1907.

## CHAPTER XIV

1. *Die Wissensformen und die Gesellschaft*, Leipzig, 1926.
2. This point is duly emphasized in Gruenwald's *Das Problem der Soziologie des Wissens*, Vienna, 1934. I have learned a good deal



from this remarkable book, which was edited by W. Eckstein. Its author, a Viennese sociologist and philosopher, had fallen victim to an accident in the mountains at the age of 21.

3. All quotations are from *Ideology and Utopia, An Introduction to the Sociology of Knowledge*, with a preface by L. Wirth, New York, 1936. His later published work, *Man and Society in an Age of Reconstruction*, New York, 1940, does not show relevant modifications of his view with regard to the points at issue.
4. Ibid. p. 240.
5. Ibid. p. 241.
6. Ibid. pp. 262 f.
7. Ibid. p. 263.
8. Ibid. p. 259.
9. Ibid. p. 261.
10. Ibid. p. 269.
11. Ibid. p. 270.
12. Ibid. p. 271.
13. Ibid. p. 266.
14. Ibid. p. 270.
15. This, incidentally, is the reason why Kant would reserve the title of 'genius' for the great artist. Cf. *Kritik der Urteilkraft*, §§ 46 and 47.
16. Cf. the recent interesting discussion of the pertinent problems by F. J. Teggart, M. R. Cohen, and M. Mandelbaum in the *Journ. of the History of Ideas*, Vol. III, No. 1, 1942; and the article by C. G. Hempel, 'The Function of General Laws in History,' *Journ. of Phil.*, Vol. XXXIX, No. 2.

#### CHAPTER XV

1. 'Der Sinn der Wertfreiheit der soziologischen und ökonomischen Wissenschaften' in *Gesammelte Aufsätze zur Wissenschaftslehre*, Tübingen, 1922, pp. 451-502, p. 470.
2. Cf. the lucid presentation of the issue in Dewey's 'Theory of Valuation,' *Intern. Encycl. of Unified Science*, Vol. II, No. 4, pp. 40 ff.
3. This is required in an examination of Durkheim's works, for example.
4. Cf. his *Reine Rechtslehre*, Vienna, 1934, which contains a summary of his doctrine and a large bibliography (by R. A. Métall) of the writings on this subject. My books *Logik und Rechtswissenschaft*, Tübingen, 1922, and *Die Kriterien des Rechts*, Tübingen, 1924, deal with the logical foundations of Kelsen's theory.

5. Cf. *Reine Rechtslehre*, pp. 62 ff.
6. Cf. my *Methodenlehre der Sozialwissenschaften*, pp. 291 ff.
7. Cf. *Die Grenzen der Naturwissenschaftlichen Begriffsbildung*.

## CHAPTER XVI

1. J. S. Mill, *Essays on Some Unsettled Questions of Political Economy*, 3rd ed., 1877, pp. 138 ff.
2. Cf. T. W. Hutchinson, *The Significance and Basic Postulates of Economic Theory*, London, 1938, pp. 40 ff. Cf. also the criticism by F. H. Knight in *Journ. of Polit. Economy*, Vol. 48, February 1941; Hutchinson's reply and Knight's rejoinder, *ibid.* Vol. 49, October 1941; my article 'On the Postulates of Economic Theory' (dealing with this controversy) in *Social Research*, September 1942; and A. Lowe's article, 'A Reconsideration of the Law of Supply and Demand,' *ibid.*, November 1942.
3. *Recherches sur les Principes Mathématiques de la Théorie des Richesses*, 1838, Ch. v.
4. Cf. O. Morgenstern, 'Vollkommene Voraussicht und Wirtschaftliches Gleichgewicht,' *Ztschr. f. Nationalökonomie*, Band vi, 1935; and Hutchinson, *op. cit.* pp. 94 ff.
5. The following analysis refers to the Austrian School of Marginal Utility based on C. Menger's *Grundsätze der Volkswirtschaftslehre*, 1871; St. Jevons' *Theory of Political Economy*, 1871; L. Walras' *Éléments d'Economie Politique Pure*, 1874; and J. B. Clark's *Philosophy of Value*, 1881, laid down similar ideas. I have given a more detailed methodological analysis of the basic ideas of the marginal-utility school in my *Methodenlehre*, pp. 255-90.
6. The measurability of marginal utilities has been asserted by C. Menger, Jevons, Walras, Edgeworth, Marshall, J. B. Clark, Wicksell, Schumpeter, Frisch, and many others. It has been denied by Cuhel, Bilimowitsch, Pareto (in his writings after 1900), and others. Cf. Article 'Grenznutzen' by P. Rosenstein-Rodan in *Handwoerterbuch der Staatswissenschaften*, Vol. iv, 1927, pp. 1190-1223, particularly p. 1193.
7. *Grundprobleme der Nationalökonomie*, Jena, 1933, p. 22.
8. *Ibid.* p. 22 f.
9. *Ibid.* p. 23.
10. *Ibid.* p. 24.
11. *Ibid.* p. 25.
12. Cf. p. 208 ff.

## CHAPTER XVII

1. Cf. K. Pearson, *The Grammar of Science*, Introductory, § 11: 'Hundreds of men have allowed their imagination to solve the universe, but the men who have contributed to our real understanding of natural phenomena have been those who were unstinting in their application of criticism to the product of their imaginations. It is such criticism which is the essence of the scientific use of the imagination, which is, indeed, the very life-blood of science.'